

3 1761 05294190 3







Digitized by the Internet Archive
in 2007 with funding from
Microsoft Corporation

1.
P17c1

THE ASCLEPIAD.

A Book of Original Research and Observation

IN THE

SCIENCE, ART, AND LITERATURE OF MEDICINE,
PREVENTIVE AND CURATIVE.

1890.

BY

Benjamin Ward Richardson, M.D., F.R.S.

VOLUME VII.

TERAR DUM PROSIM.

London :

LONGMANS, GREEN, AND COMPANY,

PATERNOSTER ROW.

P. BLAKISTON, SON, & CO., PHILADELPHIA, U.S.A.

CUPPLES, UPHAM, & CO., BOSTON, U.S.A.

149 761
115719



To

WILLIAM MUNK, M.D., F.R.C.P., F.S.A.,

HARVEIAN LIBRARIAN AND AUTHOR OF THE "ROLL OF THE ROYAL COLLEGE OF PHYSICIANS,"

A COLLEAGUE WHOSE LEARNING AND SKILL

IN THE SCIENCE AND ART OF MEDICINE

IS ONLY EQUALLED

BY HIS HONOURABLE AND GENEROUS NATURE,

THIS, THE SEVENTH VOLUME OF THE "ASCLEPIAD,"

IS GRATEFULLY INSCRIBED.

PREFACE

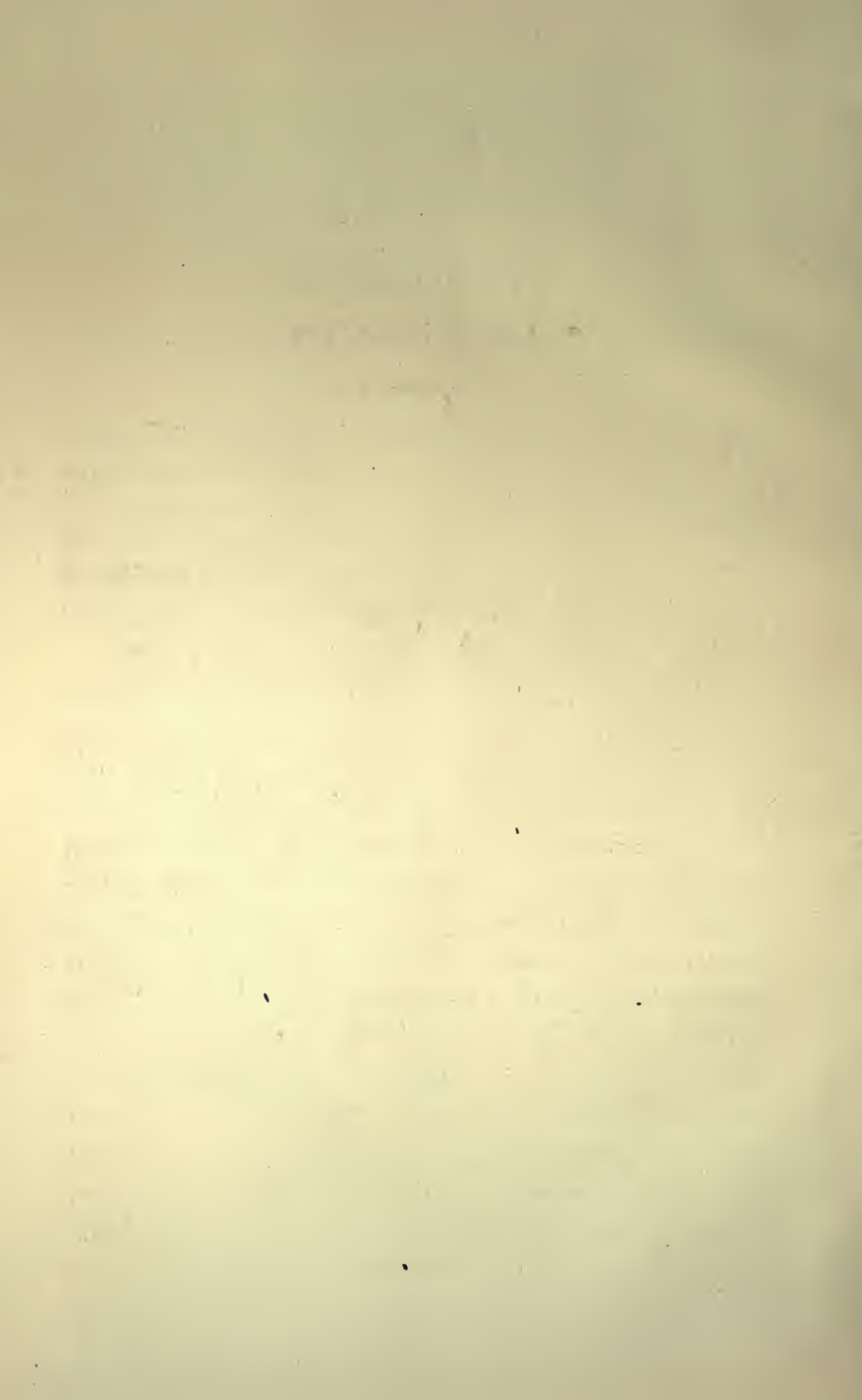
TO THE SEVENTH VOLUME OF THE "ASCLEPIAD."

WITH the publication of this volume the ASCLEPIAD has passed through what would in old times have been called its apprenticeship. It may be considered as having won its place as a regular contribution to Medical Science and Art, and I do not conceal the happiness I have experienced in writing the seven volumes, or the pleasure of knowing that they have found a place in the libraries of medical scholars of many other countries as well as my own.

In accordance with a wish largely expressed, this volume contains a series of Reports, running on from number to number, of a therapeutical research carried out many years ago. These Reports have been so well received, I shall from time to time continue them in future, perhaps not in serial form, for that is rather inconvenient, but in such way that each one will be complete in itself and brought up to current date.

BENJAMIN WARD RICHARDSON.

25, MANCHESTER SQUARE, LONDON, W.
November 15th, 1890.



CONTENTS.

VOLUME VII.

	PAGE
ON DEATH BY CHLOROFORM	1
CYCLING AND PHYSIQUE	21
OPUSCULA PRACTICA	40, 136, 240, 328
MONRO, ALEXANDER, M.D., F.R.S. (<i>with Portrait</i>)	49
REPORTS PHYSIOLOGICAL AND THERAPEUTICAL MADE BY THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FROM 1863 TO 1872 :—	
ON THE PHYSIOLOGICAL PROPERTIES OF NITRITE OF AMYL	68, 178
„ „ „ „ NITRATE OF AMYL	283
„ „ „ „ COMPOUNDS OF METHYL AND ETHYL	361
COTEMPORARY PRACTICE AND LITERATURE	88, 190, 295, 387
ASPHYXIA, APNŒA, AND SYNCOPE OF THE LESSER CIRCULATION	101
THE HYPNOTIC EPIDEMIC	118
CULLEN, WILLIAM, M.D., F.R.S. (<i>with Portrait</i>)	148
ARTIFICIAL RESPIRATION : THE THEORY AND PRACTICE	201
LESSONS FROM THE FASTING MANIA	225
BLACK, JOSEPH, M.D. (<i>with Portrait</i>)	254
EXECUTION BY ELECTRICITY	291
CENTENARIAN PATHOLOGY	301
MEDICAL TEACHING, OLD AND NEW	317
BELL, BENJAMIN, F.R.S.E. (<i>with Portrait</i>)	336
PULMONARY CONSUMPTION	381

ILLUSTRATIONS.

VOLUME VII.

	PAGE
MONRO, ALEXANDER, M.D., F.R.S. <i>Portrait facing</i>	49
CULLEN, WILLIAM, M.D., F.R.S. „ „	148
PORTRAIT OF CULLEN, BY KAY „	175
BURIAL-PLACE OF CULLEN	177
BELLOWS FOR ARTIFICIAL RESPIRATION	211, 212
BLACK, JOSEPH, M.D. <i>Portrait facing</i>	254
PULSE TRACING OF A PERSON 102 YEARS OLD	308
PULSE TRACING OF A PERSON PREMATURELY AGED FROM ALCO- HOLIC INDULGENCE	309
BELL, BENJAMIN, F.R.S.E.. <i>Portrait facing</i>	336
MOORE'S APPARATUS FOR LOCAL ANÆSTHESIA BY PRESSURE <i>Facing</i>	355



ON DEATH BY CHLOROFORM.

A SCIENTIFIC Commission sitting at Hyderabad has come to the conclusion, after an elaborate and painstaking inquiry, that when chloroform, on its inhalation, proves fatal, the cause of death is, primarily, arrest of the respiratory function, or, as it is more briefly expressed, "by asphyxia." To the work of that Commission the utmost respect must be paid. It has been a most honest, a most laborious, and a most conscientious Commission, and every detail of observed fact which it has recorded may be received without a moment of hesitation. With all this ready and proper admission, the results at which the Commission has arrived appear to me, one of the earliest and oldest of the experimentalists on the action of chloroform, to be likely to lead to certain anticipations which future events relating to mortality from chloroform will not confirm. I proceed to show, as shortly and clearly as I can, on what grounds this statement rests, and in order to place the argument in a concise form, I shall treat it under three heads. Under the first head, the subject of *comparative death by chloroform*, by which must be understood the mode of death by chloroform in the inferior animals, as compared with death by the same agent in man, will be discussed.

Under the second head, *physiological death by chloroform*, that is to say, the mode of death by chloroform when the death is uncomplicated, will be dealt with. And, under the third head, *pathological death by chloroform*, or death by chloroform under various conditions and forms of disease, will be considered.

COMPARATIVE DEATH BY CHLOROFORM.

In studying the subject of comparative death by chloroform, which mode of study forms the staple of the work of the Hyderabad Commission, certain inferences admit of being drawn which cannot hold good for all mankind, because of the differences existing between the subjects experimented upon and the human subject. These differences are (a) mental, (b) physical, and (c) operative or accidental.

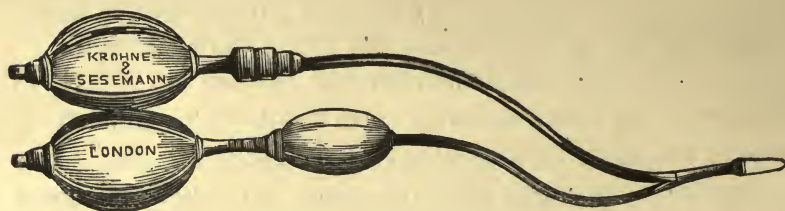
Mental Differences. The Element of Fear.

The first failure of inference from the results of the Commission will, I think, arise from the fact that the reasoning soul, as Thomas Willis called it, is left out of the argument. I mean by this, that the main line of research has been made on animals inferior to man; animals wanting in that reasoning soul or intelligent mind, which not only remembers the past but forecasts on the past for the present and the future, the influence of which all-influential part of the human animal is immediately and importantly related to the results of the administration of a narcotic vapour. When an inferior animal is brought under the influence of chloroform it has no foreknowledge of the risk which it has to undergo, has no human anxiety or fear, telling upon its heart and circulation. The lower animal goes to sleep without the preliminary suspicion that it may never wake again. It sleeps and wakes mechanically. But the human

being is, in this respect, not so favoured. It has heard of the danger into which it is about to pass, and although the vast majority of human beings think little of the danger, there is always a small minority that does; and, in this minority, the thought is a danger which, whether expressed or concealed, is of serious consequence. The factor of fear, in such cases, is one of the chief dangers to the circulation, and is entirely unaccounted for in conclusions drawn from anæsthesia induced in the inferior animals. It is only in the case of very young children that this factor can be excluded; in their case the exclusion comes in the same way as in the lower animals, and we know that death from chloroform in the very young, of human kind, in whom reasoning fear is excluded, is the rarest of occurrences.

In mature life the element of fear leading up, easily, to failure of the heart, as chloroform begins to be inhaled, is one of the most serious elements of danger in the whole range of chloroform administration. In such instances it is of comparatively small account where the first embarrassment, from chloroform pure and simple, in a physiological point of view, commences; for the fatal mischief is done before the phenomena of asphyxia, in the true sense of the word, are seen, or in any way declared. So it happens that in most examples of death under chloroform, and under all the volatile narcotics belonging to the chlorine family, the act of death is quick and complete, the wrist pulse stopping dead before the respiratory movements have ceased. They, who have kept records of the reported modes of death in the human subject under chloroform, know that this is the common mode of death; and they also know that this is the reason why, when other rapid and fatal symptoms are developed, restorative means are so often ineffective, even when resorted to immediately. If death under chloroform

always took place, as it so often does in the healthy inferior animal, from the continued inhalation of the vapour until respiration ceases, then death would never occur if artificial respiration were promptly set up. In my demonstrations on the administration of chloroform to inferior animals, in order to show resuscitation under artificial respiration, I have brought back life, with the double acting bellows depicted below, many times after death seemed to have been fully pronounced for five and, in one instance, nine minutes. In such instances a heart in a healthy condition was still acting in the low degree of tension which I have designated the reduced



or cataleptic degree; and when by the artificial respiration the blood was freed to some extent of its embarrassment in passing through the lungs, the heart resumed its full swing, the respiratory muscles regained their power, and the whole body was reanimated. This is plain sailing in resuscitation, and may be attended by results which, to an outside observer who has not got all the facts in his possession, seem almost like phenomena of a miraculous order.* Such phenomena, however, have no strict rela-

* At the meeting of the British Medical Association at Leeds in 1868 a gentleman, whilst delivering a very able lecture on anæsthesia, put a rabbit so profoundly asleep with nitrous oxide gas that it passed for dead. I was unfortunate in having mistaken the hour of the meeting, and did not arrive until the lecture had just come to a close; but on my arrival one of my colleagues pointed to the animal on the table,

tionship to death in the human subject in which the effect of mental shock, now under consideration, is doing its fatal work; in which a feeble heart, fainting already, faints outright in the preliminary stage of narcotism; and in which death is too often absolute, owing to cardiac as well as respiratory failure. In such examples, although the primary failure be in the respiration, the extension of the shock to the heart is too rapid to give time for any artificial respiratory movement that would bring back animation if the heart were unaffected.

I can but fear that instances of death under chloroform will continue to occur as frequently as ever under the constitutional conditions above specified. In many of

supposed to be dead, with the remark that it was a good subject on which to try the method of resuscitation I had been descanting on a few hours previously in one of the Sections. I did not know how long the animal had lain insensible, but having the apparatus for artificial respiration in my pocket I went down to the table, and, with the permission of the lecturer, set up the double acting artificial respiration. After sustaining this action for about four minutes I noticed the usual returning muscular movements in the flank muscles which precede signs of recovery in such cases, when recovery follows; whereupon I ceased working the bellows, and informed the audience that the animal had now regained its independent life, and would recover. I shall never forget the general expression of incredulity with which this statement was received, or the strong word of the same import which I heard whispered at my back by a rank unbeliever there. But when the facts became clear, when the animal began to breathe again on its own account, as it soon did, when it rose on its legs, and when it ran about the table quite restored, looking as if there had never been an interruption in its ordinary life, the signs of incredulity passed into those of wonder quite as startling. I explained, of course, that this animal, although it would have died outright if it had been left to its fate, was really not actually dead when I commenced my operations upon it, because its heart was still imperceptibly working at low tension; but even that explanation did not altogether remove the idea of the marvellous from some impressionable minds, and ten years afterwards one of the lookers-on told me that the event, explain it how I might, had left on him a sensation of wonder which was still fresh in his mind.

such cases the fatal event has taken place after the vapour from a few minims of chloroform has been inhaled; and in most instances the death has been pronounced in a shorter period than that during which a person diving under water would easily hold the breath. In these cases it cannot be said that death, in the strict sense of the word, is brought about by asphyxia. The seat of death was in the heart, even though the fatal aberration started from the respiration, and though in a perfectly constituted organism there would have been no death except through respiratory failure.

Physical Differences. The Morituri.

Another error, derivable from any argument or inference resting exclusively on lines of experiment with chloroform on the inferior animals, arises from the fact that a great group of the human family—a group I have on a previous occasion called the *Morituri*—is excluded from the argument. By the *Morituri* I mean those of the human family who are in such close proximity with death that they are at any moment ready to die from the most trifling external causes, and who constitute the numbers that fall down in sudden death. Amongst the lower animals, excepting those which have come to the very close of their lives, from extreme old age, and excepting those that are driven, by absolute pressure of work, or by starvation, into exhaustion, the *Morituri* are practically unknown. But in the human species there are few families in civilised communities which have not, in the course of a single generation, afforded an example of some member of the class specified. In every crowd or assembly of human beings, made up of three or four thousand people of adult age, there will almost certainly be one at least in such proximity with

death, that he or she would die if brought, even by the most skilful and careful administration, under the influence of chloroform. Moreover, the cause of the proximity with death is so varied that it is difficult, if not impossible, to provide against the danger. In one it will be dread; in another, passion over which the reason has imperfect control; in a third, some organic feebleness of the heart, incident to muscular degeneration; in a fourth, a disturbed balance between the respiratory and circulatory systems; in a fifth, some cerebral disturbance or deficiency; in a sixth, some physical peculiarity of the blood, which may modify the absorption of the narcotic vapour, or cause it to damage the function of the vital organs. In the most susceptible class of all, the ganglionic nervous system, a system, the pathology of which is still very imperfectly understood, is the centre, in the human subject, most readily disturbed by the narcotic influence, and the grand centre of failure. The frequent occurrence of vomiting under chloroform, a symptom which has no direct relationship to asphyxia, is a strong proof of this fact. As a rule, the first sign of vomiting is accompanied by symptoms of pallor and of syncope, carried, as I have often seen, in chloroform administration, so close to fatal syncope that none who observe closely can fail to observe the extreme danger. Between the ganglionic shock which produces vomiting and that which produces arrest in the motion of the heart the line is fine indeed; it is all but imperceptible.

From the first week that chloroform was brought into practice unto the present hour I have read all the available reports of death from chloroform. I had the privilege of assisting Snow in the collection of the data published in his posthumous work which I edited, and I am driven, from this long and unique experience, to the

conclusion that, in many of the instances of death from chloroform in man, the death has occurred amongst the *Morituri*, and from conditions in which the physiological death from chloroform, as it is seen in the lower animals, has played no such commanding part as to lead to the conclusion that death took place from uncomplicated asphyxia. Neither have the deaths been due to bad or unskilful administration. The patients have died because they were labouring under conditions of disease, latent or declared, over which the administrator had no control.

The views which I have stated in the preceding lines are not stated now for the first time. They were submitted in my lecture on death from chloroform, published in the *Medical Times and Gazette*, for May 28th, 1870, p. 574, in the following terms:—"Can we by any reasonable hypothesis explain, apart from method, the cause of the many and frequent deaths from chloroform? There seems to me to be a most reasonable hypothesis which does account for the facts. The hypothesis is simple: it is that in every case where a person has to be made so dead to the outer world that he neither sees, nor hears, nor thinks, nor feels, but hangs on the mere thread of involuntary organic function and action, he is dead in every part except in the mere parts which remain true to involuntary organic work; and as these necessary vital parts are not equally sound in all persons, therefore some persons succumb. If this be true, it is not on the percentage of chloroform inhaled that either safety or danger, in a given number of administrations, rests, but on the percentage of originally sound or unsound persons who are subjected, in a given number of cases, to the vapour of chloroform." I see no reason for modifying these conclusions after the twenty years of observation

which have elapsed since they were written ; and if any word requires to be added it is, that the failure of function in the organically disabled patient is sometimes developed long before the period of complete anæsthesia is declared,—may, indeed, be developed before sensibility is lost. Thus, in Snow's table of fifty examples of death under chloroform, nine of the patients died within the minute after the commencement of the inhalation, and four of these within a few seconds. It could hardly be said, with any fairness, that these persons died from the ordinary physiological asphyxia. They certainly did not ; they died, really, from a shock communicated to the heart, by which the circulation was arrested at its centre.

Operative or Accidental Differences. Surgical Shock.

Any correct inference as to the mode of death from chloroform administered to the human subject can only be reliable when the whole of the circumstances surrounding the administration are taken into account. To mankind chloroform is usually administered for surgical operations, and as the shock of an operation is broken by the narcotic action of the anæsthetic, much less danger is incurred than might, *à priori*, have been expected. Indeed, many deaths which might have taken place from operations carried out with all the penalties of acute agony, as in the olden time, are now performed safely, because of the immunity to pain conveyed by the anæsthetic. For example, we may be assured that the whole range of abdominal surgery would have been impossible because of the fatality, if anæsthesia had not lent its beneficent aid to the surgeon for saving the sufferings of the patient under the knife. This fact, to a large extent, counterbalances many possible evils, and invites the employment of an anæsthetic, instead of opposing it in all operations

where local anæsthesia is inapplicable. At the same time, a number of fatal risks still attend surgical operations under chloroform, which risks are dependent on the operation itself. For this reason I have always taught that, when a patient dies on the operating table after the commencement of the administration of an anæsthetic, a distinction ought to be drawn on the point whether he died *from* the anæsthetic, or simply *under* it. I have several times observed, when administering chloroform or methylene for abdominal operations, that the pulse of the patient has intermitted most seriously at the moment when the surgeon first puts his hand into the abdominal cavity. This is reflex cardiac shock under narcotism. I have observed the same phenomenon in a case where the surgeon made a free excision into the skin before the patient was completely narcotised, and where the symptoms of collapse were so alarming the operation had to be stayed until the patient had recovered from the syncope, and had been brought, by re-administration, back to anæsthesia, and into an anæsthesia which was absolute. My friend, the late Mr. Peter Marshall, had a case in which death actually, as he believed, took place from such an accidental shock. These are examples of shock under operation, and to them may be added rare instances of collapse from hæmorrhage.

On the matter of death from chloroform in the human subject, we must not forget a combination of several untoward circumstances, each of which may contribute to the fatal result. In very deep and prolonged anæsthesia, when all parts of the body are under anæsthetic influence, when all parts that can absorb the narcotic are saturated with it, and when the heart, from long circulation of it through its structure, is sharing with the voluntary muscles in the general collapse, the danger of syncope

from surgical shock is always imminent. In one instance, where I produced in a lady an extreme degree of anæsthesia, until every voluntary muscle was absolutely flaccid, in order to enable Mr. William Adams to break up some firm adhesions of the knee joint, I observed that when he took up the utterly flaccid limb, and applied the necessary jerk for snapping through the adhesions, the pulse, previously steady and good, made a complete stop, as in intermittent pulse, as if the motion of the heart had been mechanically arrested. This same phenomenon occurred as each shock was repeated, and would have led to death if the repetition had been continued many times. The phenomenon was observed on three occasions in which the operation had to be performed.

Sometimes a very sensitive person, belonging to the nervous class to which I have referred, sits down for a slight operation in a state of partial syncope. It may be said, "This is a slight and momentary operation, and there is, consequently, no need to do more than administer a very little chloroform,"—one of the worst practices, as Dr. Marshall of Mortlake has shrewdly pointed out, that could be followed. The patient submits to the inhalation with anxiety, and only from the dread of the pain. Then whilst half under the narcotic, and whilst the sensibility remains, he or she suddenly feels the acute pang of the operation, and helplessly falls into a threatened syncope, as might happen if no anæsthetic had been used at all. But here is syncope in an individual whose respiration is also embarrassed; a syncope which is just the kind of all kinds likeliest to be fatal. A large percentage of the deaths from chloroform which have taken place during small operations in the human subject, have been induced in the manner described. They are deaths commencing at the

heart: they are deaths from syncope under chloroform, due to the combined actions of the narcotic, the fear, and the surgical shock.

PHYSIOLOGICAL DEATH FROM CHLOROFORM.

Much has been written on the question of the pure or physiological mode of death from chloroform. I noticed a statement in one of our excellent medical journals, the *Illustrated Medical News*, that the new observations made at Hyderabad are a return to the original views of Dr. Patrick Black, namely, that death by chloroform takes place from primary arrest of the respiratory function, as opposed to the views of the late Dr. John Snow, that the death is from failure of the heart. It was my good fortune to know the two gentlemen named personally, and to take part with both in the early discussions on the important subject in hand; and I can state, with perfect knowledge, that they were not so far apart from each other in judgment as is supposed, when the physiological question was alone under discussion. Black was of opinion that, in the physiological death from chloroform, the respiration invariably stops before the arrest of the circulation, which view I also have always thought to be correct, and have, from the first, always taught; but I think he erred somewhat in drawing too sweeping a conclusion from one particular induction. In the course of a lecture at the College of Physicians I demonstrated before him the condition of the hearts of two animals deeply anæsthetized, the one with ether, the other with chloroform, and he admitted that the heart of the animal under ether was in a much more vital condition than that of the one under chloroform; a fact which Sir Thomas Watson, who was present, remarked, "admitted of no doubt whatever." Snow, again, to my

knowledge, was anything but resolute on the point, that in the physiological death of the lower animals under chloroform, failure of the heart is the primary cause of death. Both he and I performed many experiments in which we never failed to observe that when an animal was killed by the inhalation of chloroform, the heart, on being exposed, would beat actively after the respiration had entirely stopped. The fact was demonstrable, and he never once disputed it, but he argued that there was no proof that the action observed was sufficient to carry on the circulation. He quite admitted that in inferior animals the respiration might be the first to stop; but he was greatly bitten by Dr. Francis Sibson's ingenious idea, that as the heart is the first organ of the body to be fed with the blood charged with chloroform from the pulmonary veins, therefore it would naturally be the first organ to succumb under the influence of the anæsthetic, and would, of necessity, feel the influence before the respiratory muscles felt it. He was, however, most impressed by the phenomena of death under chloroform in the human subject, in which what may be called the pathological, as distinct from the physiological, mode of death is witnessed; and, as in the pathological mode the heart seemed to him to be most frequently the first centre to fail, he became inclined to attach a primary importance to that source of failure.

For my own part, I can only repeat, in respect to the physiological mode of death from chloroform, the exact words which I published in the *Medical Times and Gazette*, of May the 28th, 1870, in the lecture already quoted. There I describe that, in death from chloroform in an inferior animal, even though the death be very quickly brought about, the immediate cause of death is not from the absorption of the vapour into the blood, but from the

direct action of the vapour on the nervous system, the stimulation excited by the vapour causing the animal spasmodically to cease to breathe, and inducing the apnœa under which arrest of the action of the heart quickly follows. There, also, after applying the same line of reasoning to certain classes of deaths in the human subject, I proceed to observe,—

“In inspiring the vapour of chloroform, what do we do? We bring the vapour over an immense surface exposed to blood. There the vapour is condensed, there it commingles with the arterialized blood as it becomes condensed, and from thence it travels direct through the left cavities into the structure of the heart. The progress is much the same as that after its introduction through a vein; but yet I cannot, in a healthy animal, bring the heart to a standstill, in the same way, through the lungs. I always find that as the quantity of vapour is increased, the introduction of it into the blood is impeded by the direct action exerted upon the pulmonary vessels. In plain matter of fact, I doubt whether it be possible to kill a perfectly healthy heart by the direct action of a poisonous amount of chloroform introduced into its substance through the respiratory surface. But I would not say the same of a heart that was not healthy, nor of a heart in which the nervous mechanism is injured, the muscular substance appearing healthy. Such a heart might possibly succumb under a dose of chloroform small enough to pass through the pulmonary vessels without exciting contraction in them, and small enough not to excite anæsthesia.”

To the view thus enunciated twenty years ago I should to-day adhere as firmly as ever. I have never seen anything that would lead me to change it; I believe that it correctly represents the facts as to the natural

or physiological mode of death, and the researches carried out in the Hyderabad Commission seem to confirm, in the fullest degree, the selfsame conclusion. Of late years, in my inquiry as to the best means of producing a painless death or euthanasia, I have witnessed death from anæsthesia in over seven hundred dogs, and have had reports of the deaths of as many thousands, chloroform having played its part in every case. Here again the evidence has always been corroborative of the view that the death commences in the respiration, the exceptions from the rule being too few to be considered as vitiating the main conclusion. At the same time, I have seen a few very singular and instructive exceptions. I have noticed that an aged animal has died in a space of two minutes in an atmosphere of chloroform in which a young animal has remained simply asleep for the space of an hour. Once, also, in the instance of a dog that was suffering from intermittency of the heart, a condition to which dogs that have borne a great deal of labour and care are liable, the death took place so soon as the vapour began to be breathed, precisely as if from syncope, the heart coming to a dead stop, although the respiration continued. But this should perhaps be classed amongst the pathological forms of death from chloroform.

There is one other observation that requires to be recorded before taking leave of the physiological mode of death under chloroform. I have many times observed, that if a perfectly healthy animal be exposed suddenly to an atmosphere of common air, largely charged with chloroform, the respiration will be checked almost instantaneously, and death will be declared as directly due to the respiratory failure. In this condition of things, artificial respiration, though it be started at once, may

be of no avail, and the reason of failure is not far to find. On laying open the chest the lungs are found not only free of congestion, but absolutely bloodless, white, in appearance, as milk, and comparable only, in respect to loss of colour, with that deadly pallor of the fingers of the hand in some forms of disease to which, in these pages, the term of *local syncope* has been applied. In fact, there is now a local syncope of the lungs, and the blood finding no possible passage over the pulmonic circuit, the heart, much congested on its right side, is actually pressing against the pericardium and is inactive, not because its vitality has ceased, but because it is held motionless by the membranous envelope in which it is bound. So soon as it is set at liberty from the membranous compression, by opening the pericardium, (a task occasionally of much delicacy, owing to the close apposition of the two layers of pericardium), the right auricle begins to contract most vigorously; the right ventricle follows; the left ventricle and auricle may also follow; and, good cardiac movement may continue for several minutes. Some would call this mode of death death by asphyxia, and if we are always to describe death which commences at the lungs by that name, then it may stand as correct; but it is not asphyxia in the ordinary sense of the term, it is syncope commencing in the pulmonary circuit; in a word, pulmonary or apnceal syncope.

Touching the truly natural death by chloroform from asphyxia or arrest of the respiration, we have still two or three knotty points which remain to be settled. In the first place: (1) Is the action of the chloroform merely reflex from the mucous surface of the respiratory tract to the respiratory nervous centre? (2) Is it directly upon the terminals of the

pulmonary artery and the commencing branches of the pulmonary veins? (3) Is it by absorption of chloroform into the blood, and its conveyance to the respiratory nervous centre? (4) Is it by the muscles of respiration failing under the direct action of the narcotic, or by obstruction in the circuit of blood through the lungs, that the respiratory function is impeded? (5) When the respiratory function is impeded, does it ever happen that the heart itself fails to share in the embarrassment? The inference I should be led to draw from what I have seen is that in very rapid death the effect is reflex from the respiratory surface to the respiratory centre, with, in extreme cases, direct action upon the arterioles of the pulmonic circulation itself, by which a fatal obstruction to the course of the blood is, mechanically, produced.

Altogether, in thinking of death by chloroform it is essential to keep the physiological mode first in view, and to connect it with failure of respiration as its prime source of failure. The variations from pathological modifications are then better understood, and assume their true position.

PATHOLOGICAL DEATH BY CHLOROFORM.

In 1870—*Medical Times and Gazette*, July 23rd, p. 85—I defined four modes of death by chloroform in the human subject, namely, (a) by syncopal apnœa; (b) by epileptiform syncope; (c) by paralysis of the heart with paralysis of the muscular system generally; (d) by chloroform combined with surgical shock. I still maintain that these are correct definitions, for although they differ from the pure physiological mode of death, they stand out clearly in death of the human subject.

The phenomena of death under the first of these modes is the nearest to the physiological mode, and might be

called, in some senses, death by asphyxia. I defined the phenomena originally as follows. The breathing ceases, and signs of commencing apnœa, "lividity, coldness, and insensibility," succeed. "These signs appear, then the heart stops, and, if nothing be done to restore life, death is complete. When we open the body of an inferior animal after this form of death, we find the heart containing blood on both sides, but, for the moment, paralysed. We find blood in the pulmonary circuit, though the lungs may not be congested with blood; we find the voluntary muscles responsive to the electric current; and, lastly, let this fact never be forgotten, the heart, though paralysed for the moment, is not irrevocably dead: we expose it to air by opening the pericardium, and as the blood in the right auricle absorbs oxygen and brightens in colour, the auricle re-commences to contract, the ventricle follows rhythmically, and the motion of the organ is restored."

The second mode of death, in pathological form, occurs during the second stage of narcotism,—the rigid stage, as it has been designated,—when the whole muscular system, voluntary and involuntary, shares in the excitement. Efforts to vomit not infrequently precede death, and the death is strictly by epileptiform syncope. All through the body there is evidence of arterial contraction. The right cavities of the heart, the sinuses and veins, are engorged with blood, whilst the arteries are contracted and empty. It is as if the arteries had bled themselves into the veins, and had shut up the blood in the veins. In this epileptiform type of death the muscular rigidity alternates with relaxation, the result being a convulsive effect resembling epilepsy. The first case of death from chloroform in the human subject, the case of Hannah Greener, seems to have been of this character.

The third mode of death is from prolonged and gradual administration of the vapour of chloroform, by which there is produced a general paralysis of all the muscles of the body, including the heart. Death now is preceded by intermittent action of the heart, and when, in the lower animals, the condition of the heart is tested immediately after death, the heart is found irresponsive to galvanism, although the respiratory muscles and the voluntary muscles may respond to that stimulus. I cannot avoid the conclusion that in this instance the failure is from the heart.

In the fourth mode of death, from a combination of chloroform and surgical shock, the death must, I think, also be largely attributed to failure of the heart and syncope, not solely to primary failure of the respiration.

TREATMENT OF COLLAPSE FROM CHLOROFORM.

In the greater number of instances of collapse from chloroform, under any of the types described above, the collapse will be final and fatal, but some few persons may possibly be saved by one method. No doubt under a more correct knowledge than we now possess, each type ought to find its own appropriate treatment; but as yet we know only of one plan, which being applicable to all forms, is promising in some cases, and is harmful in none. That process is prompt artificial respiration, carried out, the body being at perfect rest, with double acting bellows, which by one stroke exhaust and by the other fill the chest with air. This should be carried on until the natural respiration declares the return of life, or until a coagulum of blood in a vein declares the fact of absolute death. I suggest once again that to every operating table there should be attached the proper

instruments for such immediate artificial respiration; and here, for the moment, I rest, yet not without hope of soon being able to return to the subject with a more advanced method.

One concluding word. I should write against my convictions if I said that Dr. Laurie and his learned colleagues, by their most able researches, had solved the problem of the cause of death by chloroform in the human subject, or had reduced that cause to one cause. I am sure they have not; and I fear that in this country, where in the hurry and scurry of human life so much of human infirmity finds its way to the operating table, and where the ever-varying climate makes the administration of chloroform uncertain, deaths from chloroform will still go on at the rate I estimated long ago, of one death in two thousand five hundred administrations. At the same time, I join most heartily in the well-deserved honour generally accorded to the Hyderabad Commission for their labours, with the full conviction that better observers could not have undertaken the important task, and that the choice of the *Lancet* in the English Commissioner, Dr. Lauder Brunton, was as sagacious as it was fortunate.

CYCLING AND PHYSIQUE.



FEW years ago I ventured to draw the attention of the public to the subject of cycling in its relation to health. In that effort I brought together the substance of what I had learned up to the time, but since then great changes have taken place in the construction both of bicycles and of tricycles, by which the character of the work of cycling has been materially modified, with many advantages to the rider. The class of rules for riding, and the quality of riding, has also undergone much modification: feats of skill and endurance which were never dreamed of then are now carried out in systematic style; and, altogether, the rules for health in cycling are so far modified by the differences of the art that a full revision of them is demanded.

ON CLASSES OF RIDERS AND PHYSIQUE.

Without laying it down as a hard and fast rule that riding either on the bicycle or the tricycle ought never to be commenced by boys or girls, I am satisfied that it is always best to delay the commencement of cycling until the body is closely approaching to its maturity. It should be remembered that cycling is an exercise so different to any other exercise, that it moulds the bodily framework, as it were, to its own mode of motion. This

fact is seen even in adult cyclists who are too much in the saddle, and who find it a nuisance to undertake any journey on foot which can be accomplished on the machine. These riders, in course of time, almost invariably acquire what may be called the cyclists' figure, which is not graceful, and is not indicative of the full possession of perfectly balanced bodily powers. If this perversion of physical build can occur after the body has acquired its full development, it stands to reason that the same will occur, with much more determinate results, if the exercise of cycling be carried on to any great extent at the time when the body is still growing, when all the organs of locomotion are plastic, and when irregularities of shape are not only easily induced, but are easily moulded into permanent configuration. For the reasons here stated I should not recommend cycling as a pastime of the schools, and I should not favour it as an exercise even during holiday times from school, except in the most moderate degree. Certainly anything like competitive trials, involving systematic training and rapid racing, should be earnestly condemned during the whole period when the body is passing into maturity. I include members of both sexes in this prohibition; but, as may be expected by all thoughtful people, it should be applied with greater force to girls than to boys. According to my observation the systematic pursuit of cycling should never be fully commenced until the rider has arrived at maturity, that is to say, until the age of twenty-one years has been attained. After that period the risk of any real deformity from it is over in the case of sensible people. All people, however, are not sensible on this point, for the temptation to over-cycle is a strong temptation, or, more correctly speaking, it includes a set of temptations which are apt to mislead and bring about injury. There is no

one who has ridden much who does not understand the fascination of riding when once the first fatigues of a long ride have been overcome. The body may be actually tired, and still there is often a physical pleasure in continuing the labour, with a positive sense of regret when the end of the journey is reached. I have felt this sensation myself, and am not, therefore, surprised that it should be strong in younger persons. Then, again, there is the feeling, connected with cycling, of making so much quicker passage from place to place on the cycle than on foot, with a sense of pity for the poor wretches who have to pedestrate slowly on, as if their journey would never come to an end. Lastly, there is the still worse temptation of competition, which we must candidly admit is, to almost every earnest and practised cyclist, a demon of danger which neither years, nor argument, nor self-introspection, can completely satisfy, and which I am ashamed to say is so much my master, that although I have never been committed formally to a race, I have more than once done too much in one run, or one series of runs, from the determination not to be beaten by some other rider, a bad resolve, and one so close to the racing mania that it would take a good casuist to distinguish "t'other from which." These incentives to overdo are powerful inducements towards the infliction of injury, and without any doubt they do inflict injury. Carried to an extreme they are opposed to good health, and are wont to transform a most useful and, within proper bounds, healthful exercise into a mischievous one, a mistake that ought to be sincerely regretted and seriously reformed.

Enthusiastic riders will ask, naturally enough:—In what way does physical injury occur from cycling, and what is the importance of it when it does take place? I will try to answer their question faithfully; and, inasmuch as I

am not deficient of some part of their own enthusiasm, they may be quite certain I shall speak without prejudice.

EFFECTS OF CYCLING ON THE BONY SKELETON.

The first injury inflicted is that which has been already alluded to, namely, a certain amount of derangement of the conformation of the framework of the body. A few months since that active sanitary public officer, who is always right welcome as a good and useful colleague in Hygeiopolis itself, Mr. Punch, drew a picture of a deformed cyclist, which picture conveyed, at a glance, the lesson now under exposition. The picture was of course overdrawn. The backbone of the cyclist was a little too much curved anteriorly; the scapulæ were rather too much raised; the hands were more talon-shaped than is ever seen; the leg bones were too curved, and the feet were more sharply transformed into the clutch-like hand of the baboon, than is to be found in the world of cycledom. Still, allowing for caricature, there was pourtrayed in that picture an outline not by any means unlike the disfigurement which is, sometimes, induced by too much riding, and especially by too much riding when the body is young, and when the bones are wanting the resistance which gives firmness of structure, combined with grace of form and ease of graceful movement.

The changes of form now indicated are not the pure and simple results of the posture which the body is made to assume from over-cycling, but are somewhat dependent on the machine and the mode of using it. We may take it for granted that every kind of riding which tends to throw the body forward in a bent or curved position, in a temporary stoop, will, to a certain degree, produce, in time, a fixed bend or stoop. In the very young it will do this rapidly and determinately; in the adult it will do it

slightly and less permanently; in the middle-aged it will do it slowly and rather persistently if it be once established; whilst in those who are advanced in years it will bring about more quickly than is natural that common stoop of old age, which is induced in the ordinary manner by the condensation of the cartilaginous plates interposed between the bodies of the vertebral bones that build up the spinal column and give to it its mobility and flexibility.

The modification of shape which takes place in the upper limbs is also the result of too long-continued a posture combined with a too strong and long-continued muscular strain in one direction. It will be observed that in cycling with the hands resting on the cross or steering bar there is very little muscular action of the arms, the concentration of muscular effort being to grasp the handles of the machine, and, so to speak, to hold steadily on. The flexor muscles alone being in action, this grasp has a direct effect of destroying the correct balance between the flexor and the extensor muscles, a disturbance of power greatly to the disadvantage of the extensors, which are ordinarily feebler than their opponents, and which in good muscular training are invariably brought well into play, because of their natural comparative feebleness. The effect, therefore, of the exercise of cycling on the upper extremity, the arm and forearm, is to flex or slightly bend the limb, the deformity taking place in the humerus or arm bone, and in the fingers.

Connected with the upper extremity there is, however, another modification of form, one which the artist has seized on most sharply, I mean the unnatural curve of the shoulders. This at first sight seems to be due to the forward curvature of the back or spinal column, and that curvature has indeed a part in the general effect; but more than this has to be considered. When the rider is

sitting firmly on his machine, with his hands grasping the handles, he elevates, and, for the time, fixes the shoulder blades, by which he appears to bring down his neck into his shoulders, a position very ungainly, and easily transformable from a temporary to a permanent posture. It is this posture that has led to the popular view that much cycling makes people round-shouldered; a belief, it will be seen, having a correct basis.

On the lower limbs cycling tells as markedly as it does on the spine, and as the lower limbs perform the greater part of the work, they usually feel the effects of it most distinctly. Our opponents, that is to say, those of the community at large who do not approve of cycling, and who, from not understanding its value, are given to find every possible fault in those who practise it, say that a cyclist may always be known by his walk and gait. Some cyclists are charged with being bow-legged, others with being knock-kneed. It often happens that these peculiarities of gait exist in the rider before ever he begins to ride, and then the act of riding makes any peculiarity that may pre-exist more conspicuous; and this I am inclined to think is one of the worst things that can, with a strict regard to truth, be said; because if the effect were general the peculiarities would also be general, and would be observed in every one who rides regularly, which is not the fact. But if riding brings out and exaggerates any deformity, however slight, the fact must be regretted, and means must be taken to rectify it.

First, however, we have to be sure that any real objection exists, and if it does, why it does. There is no doubt evidence can be shown that some deformity of the limbs may be induced from long-continued and hard cycling in certain riders. Cyclists themselves hold the statement to be substantially true, though they do not, or may not,

attach any great importance to it, and although they may fairly discount the outside exaggerations which relate to it. Those of us who are physicians are, also, consulted on the point; and if I were addressing a purely medical society I should be able to bring forward abundant proof that some injury cannot be ignored. Admitting some injury then, we must consider how it is brought about.

In arriving at the answer to this question we have to remember the relation of the movements of the limbs to the act of propulsion of the machine. In performing the work of cycling there is the widest possible difference of propulsion from it, compared with the work which is carried out when the body is propelled by walking. In walking the whole weight of the body is borne by the legs, whilst in riding the whole of the weight of the body is taken off the legs. The lower limbs are in this way freed from an immense amount of work, which is the chief reason why the cyclist can sustain so many more hours of work than the pedestrian. There is a still further advantage, or disadvantage according to the way we look at it, in respect to the conservation and concentration of muscular power in the lower limbs. In walking all the muscles of the body work, more or less, but in cycling the muscles of the body, other than those belonging to the lower limbs, are, comparatively, at rest. Under these circumstances the nervous energy for sustaining muscular motion is set free, mainly, in stimulating the muscles of the lower limbs into activity; and as the area of work is limited, from this cause, the nervous energy holds out longer; a fact which accounts for the tremendous endurance, at work, which some cyclists have exhibited in certain of the extraordinary feats which have lately been accomplished, and in which over three hundred miles have been traversed within twenty-four hours.

It will be seen by these observations how great is the difference between the acts of walking and cycling in a physiological point of view ; and the difference once understood, we learn the reason of many well-known but unexplained facts about muscular fatigue, to which a little later on I shall have to direct attention. What I desire at the present moment to dwell upon is, that in cycling the lower limbs, relieved of the weight of the body, are, so to speak, working independently for the time, and that the pelvis of the rider, now practically a part of the machine, is fixed to it, and is almost as rigid as itself. In this position of things the femur, or thigh bone, is placed under unusual strain. The large muscles in the fore part of the thigh, the strong muscular masses which constitute what the anatomists call the quadriceps extensor, are employed in extending or lifting up the leg at great disadvantage of leverage. They spring from the pelvis, and passing downwards along the front of the thigh bone, but free from it, are attached to the knee-pan, and by it to the leg bone, which by their contraction they raise, the thigh bone acting as a fulcrum. What the strain is on these muscles every young cyclist knows to his cost, as does every cyclist, who has been for a short time out of practice, when he recommences to ride. In my own case I have more than once been actually beaten, for a time, by the strain on these muscles, and it is not until they get a kind of extra natural power that riding is easy.

On the other side, as counterbalancing these extensor muscles, there is brought into play the group of muscles on the opposite part of the thigh, commonly called the hamstring muscles, flexors which flex the leg upon the thigh, and, using the foot as the termination of a long lever, bring down the pedal with the force which pushes on or propels

the machine. The hamstring muscles, acting in counter-part to their opponents, spring, like their opponents, from the fixed pelvis, and running down the whole length of the thigh bone terminate, or are inserted into the upper part of the bones of the leg. In their contraction, by which they draw the leg back, they also labour under great disadvantage as to leverage, but they have an advantage over the extensors, in receiving an auxiliary aid from the action of the large muscles on the posterior or back part of the leg, which muscles, getting a good hold from the femur or thigh bone, help the flexors above them. There are several other muscles concerned in side or lateral movements, and in rotating movements, to which I might refer were I giving a description, anatomically, of all the muscles employed in cycling; but it is necessary for my purpose to name only the two chief sets, at this time, in order to explain the strain on the thigh bone, and the risk to which it is subjected. It will be observed that the thigh bone all through the act of cycling is between two forces. It is the interposing bar or rod between two points of resistance, the one rigid as the machine itself, and the other resistant though not steadily fixed. The strain on this long bone is, consequently, excessive; it is a strain equivalent to the force we put on the muscles, and which is so severely felt in the early efforts of cycling. It is like the strain which we put upon a bow, when we place one end of it on the ground, and with all our force, exerted at the other end, try to bend it until the string is slipped over the notch. Fortunately, the pressure on the bone along its whole length is fairly distributed; if it were not the bone would snap across under great effort. The bone is strong enough to resist this accident, and with the majority of riders it is strong enough to resist distortion. At the same time the

balance is not quite perfect. The flexor action, that is to say, the propelling action, by which the foot is brought down with the pedal, is always somewhat preponderating, and the result is that if the bone be not fully developed, or if it be deficient in organic or mineral material, it may give way under long-continued pressure, and become curved, so that its slight natural curve, with the convex surface in front, is increased, and the deformity of curve of the limb is presented as a permanent deformity. This is the cause of the "bowleggedness" which is sometimes the result of cycling, and on which unfavourable comment is made. In the first examples which I witnessed of the change in the shape of the lower limbs here named, I thought that the change was general through the limb, and not in one particular bone. I am now convinced, by more careful observation, that the curvature is in the thigh bones alone, and that the shortening of the height of the body which accompanies it, is simply due to the curvature of these bones.

Now and then, instead of the change above related, the lower limbs are rendered what is called "knock-kneed." As explained some time ago, this is never a pure result of cycling, but is an exaggeration induced by the exercise of a natural tendency to the deformity. In the act of cycling, as it is now practised, the tendency of the rider is to bring the knees near together in the downward stroke, and at the same time to throw out the feet; a position identical in a minor degree with the distorted condition called knock-knee. This is brought about by the action of the sartorius muscle, or tailors' muscle, and of the three great muscles which draw the knees together called adductors. Men who ride much on horseback, if they are good riders, and keep secure on the back of the animal by the grip of the knee, use these muscles very

energetically, and not unfrequently become also subject to the distortion now being considered.

The last modification occurring in the skeleton from cycling is in the foot, and consists in deepening of the arch with undue curvature downwards of the small or metatarsal bones, and the phalanges of the toes. With this change the heel bone, or os calcis, may be somewhat drawn up, the whole set of changes rendering the foot less pliable and firm in walking than it would otherwise be.

I have now described the injurious effects I have so far observed on the skeleton as a result of cycling. To guard myself from any possibility of over statement, it is necessary for me to add that, taking riders as a whole, those who present the changes enumerated are exceptional; so exceptional that none but those who have seen large numbers of cyclists at work, and who are, by profession, capable of forming a correct knowledge on the subject, would read off the phenomena and their true meaning in reference to cause and effect.

EFFECTS OF CYCLING ON THE MUSCULAR SYSTEM.

Hitherto I have dealt with the skeleton as a seat of possible injury. I ought to say a word or two, next, respecting the muscles. I do not recall seeing any special functional injury of the muscles as a result of cycling. I never knew cramp or spasm as a direct result. I have never heard of rupture of muscles or of the fascia surrounding them, not even when they have been most tried. Sprain, which is a common accident from many exercises, is most uncommon as a result from working the machine, when it is separated from collisions and falls, with which I have, at this moment, no concern. I have never met with a single instance in which I could trace

the common accident of ordinary rupture, or hernia, to the exercise, although in the first days of cycling this accident was named as one which would of itself, from its frequency, put the exercise out of the field in a very short time. The evidence so far is favourable to the exercise generally, but there are one or two drawbacks which must be named.

The muscles most severely taxed attain sometimes an unusual size. The muscles which draw the upper limbs down to the sides, become, in some riders, very large and strong. The extensor muscles of the thigh are still more apt to become large and powerful, and are, I believe, always extra powerful in those who are fully trained. The muscles of the calf of the leg are also rendered large, the soleus specially, after severe and long-continued cycling exercise. These effects are the common ones, but there are exceptions even to them, for I have once or twice known wasting of the much used muscles, a bad indication, and one which should be accepted as giving notice that there is something constitutionally wrong in the person who shows it. Taking into careful consideration the general effects of cycling on the muscles which it calls most into play, we may consider that enlargement of muscle is the most prominent derangement, that such enlargement is chiefly confined to the muscles of the lower limbs, and that the effect is to create a rather indifferent gait, which, in persistent riders, is not concealed. The step loses in firmness, in walking the foot is raised rather more than is natural, and there is a tendency to tread too much on the toe in the act of walking. I cannot suppress the suspicion, also, that much cycling reduces pedestrian powers, both as regards speed and endurance. For this reason some good pedestrians who are proud of their feats on foot, or are fond

of walking exercise, have given up cycling because it spoiled their walking. The fact is not peculiar to cycling; it extends, likewise, to horse exercise, which, carried out as a habit, interferes with exercise on foot. So one of our great military surgeons once reported his opinion, which is, I believe, accepted as correct in military circles, that it is easy to turn infantry into cavalry, but to turn cavalry into infantry is one of the most difficult tasks.

The concentration of muscular power in the muscles of the lower limbs has another effect which should not be forgotten. It wearies out one particular set of muscles at the expense of all the rest; that is to say, it draws the nervous stimulus to muscular exertion from the general muscular system to divert it towards one special set of muscular organs. We must not be blind to the truth that this is a condition not altogether fair to the system at large, and I know that it is sometimes a cause of systemic disturbance. All the muscles of the body, voluntary and involuntary, call for action derived from nervous stimulus, and if they are deprived of their stimulus, by diversion of it to other parts, they undergo enfeeblement, and are not ready to perform their duty in a satisfactory manner when called upon. The fact is often well illustrated in some unprecedented and, I venture to think, unwarranted attempts to ride very long distances against time. We read that during these attempts the capacity for taking food is interfered with, that there is nausea, or even vomiting. The explanation is simplicity itself. Under the extreme exertion the digestive system is robbed of its nervous power, and from the enfeebled condition induced in it, it is unable to carry on the digestive functions which are essential for the proper digestion of foods. I have known the same thing to happen in the case of ordinary riders. A man

starts out for too long a ride without being provided with sufficient food. In time he feels a hunger which he cannot, at the moment, satisfy. He says to himself, "I am very hungry, I must push on to a place where I can get food." He pushes on; but, by-and-bye, to his surprise, the keen sense of hunger passes away, whilst the sense of exhaustion, minute by minute, increases. At last, when he reaches his destination, and the food is actually before him, he cannot enjoy it; the nervous power required for his stomach has escaped by his legs, and has been dissipated into space in the revolution of the wheels of his machine. In plain terms he has left his digestion flying behind him, and he has before him a bad and often long-continued fit of indigestion, which he attributes to cycling, but which really is attributable to inattention in a matter of common sense.

The bad effect of over fatigue in cycling on the muscles which supply and govern the organs of the vegetative life, is, or may be, supplemented by a disturbance of the muscles of the voluntary system which have, so to speak, been lying fallow. That natural and general fatigue which would lead to comfortable repose is thereby prevented. The over-wearied cyclist lays himself down to rest with the idea that he is going to fall at once into the most peaceful slumber. He does not. He is tired, but he is also restless. He turns over and over again, but only to sleep in snatches; his lower limbs sleep, but the rest of his body is but half sleeping, and when he rises, although he can ride again fairly, in so far as the work of his legs is concerned, he is weary of body, and often for some hours is discontented and not up to the mark. This state is produced by precisely the same break of nervous balance as that which has been described above. During the night

the over-exhausted lower limbs have been recuperated at the expense of the other parts of the vital machinery, the brain, the digestive system, and the other great groups of muscular structure, including the muscles of respiration, and the heart itself. So the man rises leg recruited, but not recruited through and through, as he ought to be in order to be in first-rate condition, and feeling all over in rude and vigorous health.

I have said that it has not befallen me to witness any particular strain or mechanical injury to the muscles under work by the cyclist. I have, however, a few times observed a kind of rheumatic condition induced in the muscles, attended with considerable pain, and very disabling so long as it lasts. The symptoms do not usually come on during the very act of cycling, but perhaps some hours afterwards, and they last for several hours, it may be days. The attack is always attended with feebleness and depression, and may be sufficient to lead those who feel it severely to give up the exercise, since the effect is not controlled by moderate riding, but, being constitutional, may follow a short as well as a long ride.

PRACTICAL CONSIDERATIONS.

I am now brought to the description of those practical considerations which ought to follow a series of criticisms such as have been laid down in the preceding pages; since every critic on a practical art should be ready to suggest amendments which may come into immediate use.

Firstly, I can have no hesitation in repeating, with all the power I possess, the lesson that competitive cycling is very bad exercise for those who have not arrived at full maturity, and in whom the consolidation of the skeleton is incomplete. This lesson applies to schools, to families, and to all who have the charge of the young, rather than to the

young themselves, who will not use their judgment, unless it be directed, to avoid the hurtful consequences.

The same remark applies, in a special manner, to those employers of labour who have introduced the cycle as a carrier of heavy loads from place to place, and often against time. Lads employed in this kind of service are wrongly employed; and already amongst such young persons the bad beginning of a bad system is noticeable, one so bad that it is sure to bring industrial cycling into disrepute if it be allowed to go on. To put the matter shortly, it will manufacture a set of pathological living specimens, and will, in this manner, write down its own condemnation, a condemnation which, extending to cycling altogether, will soon be greatly exaggerated, and will cause a good thing to become unpopular and out of joint with the time.

Secondly, riders of all classes should be careful not to let cycling exercise overtop altogether the natural exercise of walking. The two exercises should be combined, and, in touring, this plan ought to be rigorously followed in proper form. The proper form is, to walk ascents, and *always* to walk *steep* ascents. But it is also a good rule, even when the course is fairly level, to dismount, at times, and take a spell of pedestrian work. By this practice disused muscles are brought into play; the muscles used in cycling are allowed to rest; no time is sacrificed in the long run; and the healthy condition of body incident to pedestrian exercise is sustained. My own experience is, that for every eight miles of distance traversed for health's sake, or rather in conformity with health, it is wise to do one mile on foot. In a ride of fifty miles in a day, six at least should be done on foot, after which the repose that is demanded is equalised and nourishing, and the body is saved from that continuous strain in one position and in one form of motion, which is so injurious to physical well-being.

Thirdly, the mode in which the body is placed in the saddle during the process of riding the cycle is of great moment. The new form of tricycle with the bicycle cross steering handles, while it is undoubtedly a machine possessing many advantages, has a good deal to answer for in regard to false position of the body. The tendency of it is to cause the rider to bend forward, and maintain a severe stooping position. It is a very bad plan indeed, and I think I should say it is a very bad habit, for it is, after all, habit rather than necessity. If the bar handles are sufficiently raised to allow the body to sit upright there may be a little sacrifice of power, but that is fully compensated for by the healthiness of the natural posture.

Fourthly, we ought all to join in discountenancing those excessive competitive feats which give rise, at the time of their execution, to conditions of body so self-obviously out of the order of Nature, and against her divine ordinations. It is necessary to stand out of the world of the cyclist, as well as in it, to appreciate the disastrous influence produced by some of the published records of the so-called great feats of cycling skill and effort which have, of late, been published. One of these records, in which it was told that the competitor against time was suffering all the journey with a species of sea-sickness, so that he could not take proper food, and at times had to pursue his wild course in a kind of temporary blind unconsciousness, under the guidance of some friendly aid, created quite a shock of repulsion to the exercise in large sections of the thinking community. Such displays ought not to be encouraged; they do no real good, they are purely phenomenal in their character, and I am bold enough to say, from my professional vantage-ground, they do unquestionable injury to the future stock of health of those who carry them out. It should never be forgotten that as each person is only

endowed with a certain capital of vitality, so much of that capital as is exhausted by him is lost. There is no going back for more capital, no making up, no, not even by rest, for the prime loss from the original capacity. The most and the best that can be effected is to keep on, with a measure of the original store of energy dissipated for ever. In this respect man is just the same as the mother earth which bore him. How could he be otherwise?

Fifthly, it is of the utmost importance during exercise to keep digestion well in advance of work. The old, old fable of the belly and the members was never more appropriate than in this matter of cycling. The cyclist should never overply himself with food, but he should also never feel the want of it. If failure of power once begins to be experienced by sense of exhaustion in the region of the stomach, that hint should be acted upon instantly. It says, "Rest for a light and wholesome meal."

Sixthly, if in any rider there should come on those signs of muscular failure or change of function which I described when speaking of muscular action there must be no hesitation about giving up the exercise. The rheumatic pains may demand merely a temporary cessation of cycling, but wasting suggests, usually, cessation for good.

These are the six chief points of reform in cycling so as to make it conform with good health, and these are, in my view, immediately practical. There still remain many other topics that have to be solved before the art of cycling is perfect, on one of which I will conclude.

We have made a great point of advancement in showing what we can do with the present form of machine running on low wheels. But we still maintain a basic error in the machine, by having it so constructed that the pelvis of the rider becomes a fixed part of the machine. This is well

shown when the cyclist has to meet a hill. In climbing we push the machine, or drag it. The weight of the machine and the weight of our own bodies is precisely the same on foot as when we are riding, and yet we do easily on foot what we cannot do when we are mounted. Why? Because in riding we have not at command the power of those lumbar and gluteal muscles, which avail us so well when they are as free to work, as they are when we are on foot. In the saddle, we sit on these powerful muscles, and therefore we cannot work them; all we can do to bring these muscles into action is to bend forward and bring the feet back to the fullest possible extent. At the best the result is most imperfect. We want, in fact, two entire changes in construction of the machine, one by which we can bring the whole weight of the body into the propulsion; the other by which we can call forth all that muscular power which is used with such effect in walking and running, but is lost in cycling. If these two objects were attained, and there is not the slightest reason why they should not be attained, climbing would be just as easy on the machine as it is off it; while the degree of speed that would be rendered applicable would, at least, be doubled; that is to say, if now in ordinary riding the four miles an hour of the pedestrian is changed into eight, it would then, with the same amount of exertion, be turned into sixteen; whilst the twenty miles an hour of the fastest rider would be turned into forty, if that were a safe pace to travel.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

THE LATE PREVAILING EPIDEMIC OF INFLUENZA, AND PNEUMO-PARESIS.

HE late widespread epidemic of influenza must have recalled to all of us who can look back long enough, the epidemic of the same disease in the year 1847. I was assisting a very busy practitioner of physic—the late Mr. Thomas Brown—at that time, in the town of Saffron Walden in Essex, and such was the extent of the epidemic around us, that one day I had to ride on horseback sixty miles, going to some places at a considerable distance twice in the day, in response to the demands that were made by the sick under our care. The analogies between that epidemic and the present outbreak were alike in many particulars.

(1) *As to rapidity of attack.* The epidemic then, as now, swept over us all at once. It did not spread from point to point, but came altogether, many persons being taken with it in the same day, even in the same hour, and often instantaneously.

(2) *In regard to selection of subjects.* The disease affected most frequently adults who were in the prime or middle

of life, and amongst these, they who were most weakly, they who were most exposed to cold, or they who were suffering from mental or bodily exhaustion, or from anxiety. Women who were confined to the house were less affected than men, and aged people of both sexes who were much within doors were largely exempt. Children were comparatively exempt, as in the present epidemic.

(3) *In regard to symptoms.* The symptoms were really identical, as a whole, with those which have been lately witnessed, namely, pain in the back and limbs, headache, nausea, in some instances vomiting, fever but not of an extreme degree, and in all cases an intense prostration. To these symptoms were sometimes added soreness of the throat, and cough, with irritation of the mucous membranes, intestinal disturbance, and, in very bad types, congestion of the lungs, ending in what was called in 1847 "asthenic pneumonia," which sometimes proved fatal.

(4) *In regard to duration of symptoms in individual cases.* The duration of the symptoms in individual cases was comparatively short. Often the whole period of sickness did not exceed seven days, and twenty-one days might fairly be set down as the longest period. In no case in either epidemic was there anything like a true zymotic course of events, followed by definite sequelæ, as in scarlet fever, or measles, or small-pox.

(5) *In regard to diagnosis.* In 1847, as now, care was required in diagnosis. Many persons suffering from ordinary cold, or from bronchitis, or other chest affection, thought themselves attacked with the disease, but were quite mistaken. From some indirect influence, however, mental or physical, it did seem as if the prevailing malady stamped an impression on other prevailing affections.

(6) *In regard to cause.* In former epidemics of influenza the idea of the majority of the members of the medical

body was that the cause of influenza should be sought for in meteorological conditions. During the present epidemic little, if any, attention has been paid to this theory, the fashion of speculation about a microbe as the cause being the favoured line of belief. All eyes have been bent on the microbe as they would now be in any form of disease whatever that should become widely spread. But the symptoms of the disease influenza are opposed to the microbe speculation itself, which assumes that when a disease is set up by the presence of a microbe the symptoms induced partake of the character of a zymosis or ferment. When this is the fact, a disease running a long and regular course is presented, in which, instead of a train of symptoms rapidly developed, and terminating with slight fever in a few days, there is a stage of slow development, with an after stage lasting many days, or even weeks, of acute symptoms, and ending either in death or in a gradual recovery. Moreover, a true zymotic malady usually spreads, obviously, by contagion, affecting persons from person to person, caught, as it is commonly expressed, passing from person to person or house to house, and not to groups of persons altogether, and sometimes to a whole household simultaneously. For my part, I cling to the old theory, and believe that we ought to seek for the cause in atmospheric variation altogether. Some weeks since Dr. Elliot, of Hull, wrote me directing attention to the singular fact that the late distinguished and original observer, Dr. Greaves, of Dublin, had defined influenza as a disease in which the leading indication was paralysis of the lungs, and suggesting that the symptoms of the present epidemic not only took that form, but were most strikingly similar to the symptoms of pneumo-paresis, which have been induced in lower animals by the inhalation of nitrite of

amyl. There is no doubt that this analogy is exceedingly striking, and there is also another of equal importance, namely, the analogy of the symptoms of influenza with the symptoms induced by the inhalation of ozone. Under ozone there follow the same signs of oppression, of pains in the head and muscles, of prostration, and, if the administration be long continued, of pulmonary congestion, and all the indications of pneumo-paresis. To my mind the idea of an electrical disturbance between the oxygen and the nitrogen of the atmosphere is a far more probable cause of influenza than the presence of a microbe, in itself of doubtful definition, and possibly a coincidence, even if it be, beyond dispute, invariably present, which is not the fact.

(7) *In regard to treatment.* The tendency of the influenza in this, as in previous epidemics, has been towards spontaneous recovery, and the greater number of sufferers from it who have been wise enough to go into a warm room at once, to take mild and nourishing food, to avoid stimulants, and to yield to absolute rest of body and mind, have speedily done well. Such as have died have succumbed, as Dr. Greaves expressed it, from a "paralysis of the lungs," or, to use the more modern term, pneumo-paresis. As medicines, the old and grandly-established *bark and ammonia* have played the truest and best parts. Under the influence of ammonia the skin has been kept freely acting, the motion of the heart and blood has been sustained, the blood has been placed in a favourable condition for retaining its fluidity, and the digestive power of the stomach, always a saving power, has not been impaired by medicinal interference.

GALVANISM AND RESUSCITATION.

I HAVE noticed, lately, references, here and there, relating to the experiments of Dr. Griswold, on the effect of the Faradic current upon the nervous supply of the heart immediately after death; and the question has been put to me whether his observations accord with certain of mine on the same lines of research, published in the lectures on death by chloroform in 1870. The answer is, that the conclusions of Griswold are identical. My researches showed that when the heart is failing in power, the effect of the Faradic current is to reduce the feeble action still more quickly, from, as it seemed to me, and still seems, exhaustion of the cardiac muscle. A similar course of experiment has recently been carried out by Messrs. A. Hare and E. Martin, and recorded in their Warren Triennial Prize Essay on "The Value of the Electrical Methods employed for the Resuscitation of Persons who have ceased Breathing." From their inquiries they have come to the conclusion that the direct effect of the galvanic current on the heart is a source of danger, and that "the only place where the positive pole could contract the diaphragm also inhibited the heart." If this last named observation be true, it confirms more powerfully than ever the original warning I gave as to the danger of employing galvanism for the direct excitation of the heart in cases of collapse and of fatal narcosis, because it extends the danger also to Faradization of the muscles of respiration, and tells that such direct excitation, applied to any part of the thoracic mechanism, is full of danger. *Excite the diaphragm and stop the heart, is the lesson set by these experimentalists.* Is this a true inference? For my part, now that attention has been drawn to it, I fear it is

true. It recalls to me, that in the experiments I made with a double acting bellows for artificial respiration, in which I conjoined a current with the action of the bellows, I got remarkable respiratory action, but with invariable failure of the cardiac movements. It recalls to me, also, that I have seen hiccup in the human subject attended with like cardiac failure; both of which series of facts support the view that excitation of the diaphragm depresses cardiac function. Dr. Hammond, in the *Journal of Nervous and Mental Disease*, endorses the above views from his own independent observation; and adds the important suggestion, that a strong Faradic current used as a counter-irritant and as a stimulant, when applied to the nipple directly over the heart, or by puncture to some nerve trunk, is of benefit by reflex stimulation. This runs with the old practice of the fillip with the end of the wet towel, and may have a certain value; how useful, it is difficult to estimate.

The grand point I wish to advance once again is the danger of the practice of galvanism. When a patient is lying in collapse from chloroform or other cause, let no one be tempted to try to resuscitate by means of the direct action of the galvanic current, either through the respiratory or the cardiac organs. If by the current we call forth active movements, either of the respiratory muscles or the heart, it is like a whip to a jaded horse at best, and merely exhausts more speedily a failing centre of force which it does nothing to re-supply, and, as we now see, with a possible antagonism of action between the effect produced on the diaphragm and the heart. The whole value of treatment in cases of the kind named rests exclusively on the teachings of experiment; and if experiment with the galvanic current is opposed to the method specified, the practitioner who knowingly employs

that method in a desperate extremity, is not only acting perversely, but wrongly and foolishly. Better do nothing, till knowledge shows the right, than do the wrong thing for the sake of doing something.

THE VENOUS TURGESCECE TEST OF DEATH.



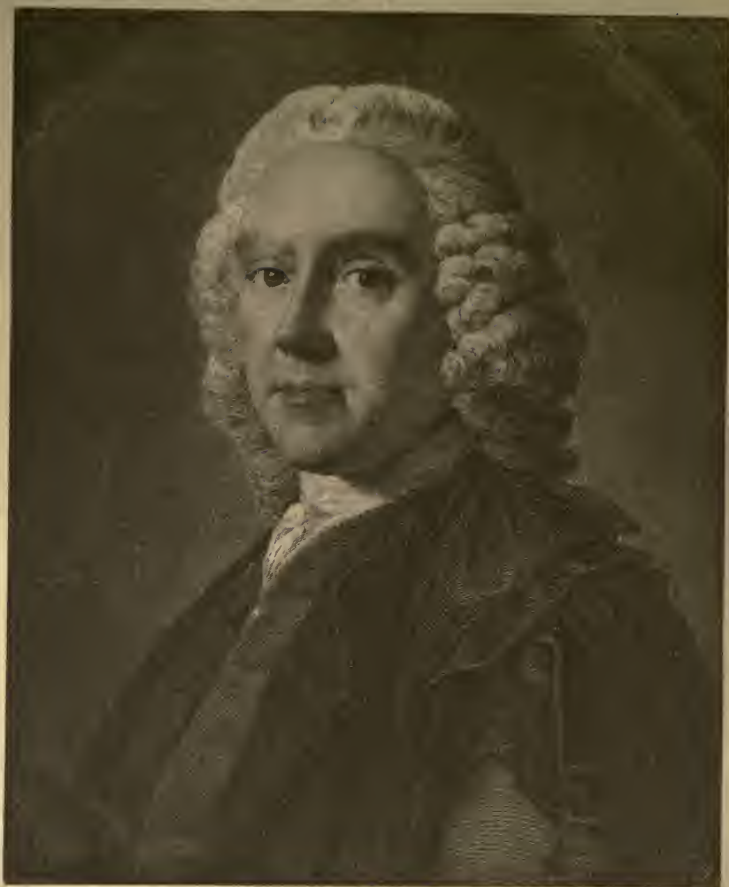
CORRESPONDENT asks me if in applying the test for proof of absolute death, by compression of the veins, it is not sufficient to put a tight band round a finger of the person supposed to be dead? The answer to that question is, that the test so applied would be of no value whatever. The test is a proof from the veins, and consists in compressing the veins so as to stop any return current of blood by them towards the heart, while the arteries are quite free. In the act of placing a tight ligature round the finger the arteries would be compressed as well as the veins, and no proof of a continued current of blood could be yielded, even if it were in progress. To carry out the method correctly it must be done in the manner described in the original paper. A short splint must be placed over the fore part of the wrist to protect the radial and ulna arteries from all pressure, and then the ligature must be firmly tied round the wrist so that the veins at the back of the wrist may be firmly compressed and the possibility of any return current by the veins proceeding from the hand entirely prevented. Should the veins on the distal side of the ligature then show the least decisive sign of turgescence, evidence is afforded of the existence of a circulation, and may be of life. On the other hand, if there be no turgescence the evidence rendered is that there is no circulatory current, and that the centre of life, the heart, is dead.

REMOVAL OF IMPACTIONS FROM THE EXTERNAL EAR.

 YOUNG man eighteen years of age was brought to me for deafness of the right ear, which deafness had been present since he was a child of about four years old, and was often attended with pain and "ear-ache." The first time the difficulty of hearing and pain was noticed was at the seaside, and came on after the child had been playing on the sands. The symptoms were attributed to cold, and were treated by the application of poultices over the affected ear, and by putting cotton wool, holding laudanum, into the meatus. After a time the acute pain subsided, but the deafness remained, and was sufficient to render hearing on the right side extremely imperfect. At the date when the patient came to me the deafness was complete on the right side, and there was a return of the aching pain. On examination the external meatus was found to be charged with wax so firmly set, that it was moved, by syringing, with difficulty. This operation brought back the ear to its usual state, but did not restore perfect hearing. The speculum now enabled me to see at the bottom of the meatus a bright white irregularly-rounded body, which, on being touched with the end of a probe, was quite resonant; and as there was no history of scarlet fever in the case, no probable disease of bone, and no indication of gouty deposits, the light began to dawn on me that perhaps the patient when a child playing by the sea had pushed a small pebbly stone into the ear. Questioned as to the possibility of this accident, he confirmed the possibility by recalling that he had the habit of "putting things into his ear," for which he had been punished by his nurse; and that he had also a notion that he did do something of the kind originally, which he was afraid at

the time to confess. Acting on this idea, I had constructed a fine wire loop of oval form fixed on a firm handle, and after two or three attempts I succeeded, after getting the loop behind the solid substance, in moving the substance from its place. Then, by the free use of the syringe, I washed out the long-impacted body, which proved to be, as suspected, a small pebble. The removal caused considerable pain, with a distressing sense of increased sounds; but in a few days my patient had regained his hearing completely.

Another instance of a similar kind occurred, during my early days, in the case of a market gardener, living near Barnes Terrace. He from childhood had suffered a long-continued deafness, for which he had been to consult the late Mr. Yearsley, who bade him come to me, his family being under my care. Thinking there was a hard mass of wax in the ear, I syringed freely, but removed nothing more than some loose particles of wax. Sufficient, however, was done to enable me to see, at the bottom of the meatus, a surface which certainly was not the tympanum. I passed a probe down to this surface, and touched it without the consciousness of the patient. I then passed down a small, finely-barbed crochet needle, and, drilling it in gently, brought it back with some fibrous substance attached to it, which the microscope told me was cotton wool. The fact led me to proceed further; I carried the crochet barb much deeper into the bed of obstructing substance, and removed a dense plug of cotton wool, which the mother of the patient had inserted during his childhood. A healthy tympanum was exposed, and in a few days this patient, after a deafness of twenty-eight years, regained his hearing perfectly.



Atclepiad No. 25, Pl. VII.

London January 1770

ALEXANDER MONRO SEN.^R

M.D. F.R.S.

*Autotype from an engraving by James Basire, 1775,
after a painting by Allan Ramsay. B.M.*

*ALEXANDER MONRO, M.D., F.R.S., AND THE
ORIGIN OF THE EDINBURGH MEDICAL
SCHOOL.*

IT is always instructive to turn to the medical school of the old University of Edinburgh, and to renew acquaintance with the great Professors there. I hope to live long enough to bring all the most famous of these illustrious men into communion with the readers of the *ASCLEPIAD*, and with that thought in my mind I cannot do better than commence with the first of the famous family of the Monros, for a long period of time the anatomical lights of the wonderful northern Asclepia of these British Isles.

Our Alexander Monro was born of an historical family. His grandfather was one Sir Alexander Monro, a brave Royalist soldier, who, with a brother David, followed Prince Charles, afterwards Charles II., to Worcester, in the ill-starred campaign against the Parliamentary forces. In the fight brother David fell, and gave up his life for his cause; but Alexander escaped, and, waiting for the restoration of the king, saw his hopes gratified. Then, tired of the military life, he sought for new honours in the profession of the law. Admitted, in due course of time, to the Bar he gained quick promotion, and became one of the principal clerks of the Court of Session.

Sir Alexander Monro had two sons, George, the elder, and John. George followed the profession of arms, and soon rose to the rank of major, after which he retired from the service, and went to reside at Auchinbowie, in the county of Stirling, where he possessed a country seat, and here he finished his days in peace and honour. John, the younger son of Sir Alexander, was brought up to medicine, and after completing the curriculum and qualification of the day in which he flourished, entered the army as a surgeon soldier, served in Flanders under King William the Third, and returned home to his wife, who was also his cousin, a niece of Mr. Forbes of Culloden, the scene of the decisive battle of that name. That he was a man favoured by some powerful patronage is shown by the fact that he was permitted in the winter months to come to London for residence, and to reside there freed, as it would seem, from the performance of official duties. It was whilst in residence in London that the subject of this memoir, Alexander Monro, was born. The birth took place on the 8th day of September—Old Style—1687.

Three years after this important family event Dr. Monro gave up his position in the army, and taking up his permanent residence in Edinburgh set up, to use the old-fashioned language, as a practitioner of physic, and very soon gained a large practice and a wide reputation. It is reported of him that he was a man of great talent and of most engaging manners. For himself he sought, however, no signal distinction. His mind was turned to the cultivation and elevation of this son, his beloved child, who from the first gave promise of great things, and who was as good as his promise, to the entire felicity of his devoted parents. From the first this boy, Alexander, was destined to the profession of physic; all the energies of his mind were turned towards it, and the desire of his heart fed

him with that attachment to the science and the art which is so essential to success.

PROFESSIONAL STUDIES.

His general education completed, the young Monro entered on his medical studies in Edinburgh, and passed successfully through them. He then was sent to London, and was placed under the tuition of three of the leading metropolitan lights of science, namely, Hawksbee, Whiston, and Cheselden. Hawksbee, remarkable as an electrician, was the discoverer of the mercurial phosphorus, and was the first man who constructed a glass globe, which he rendered partially vacuous, and then by applying his hand to it while it was in motion on its axis observed an electric light within the globe. He may, in fact, be looked on as the first philosophic discoverer of the electric light, and one of the first to make a distinction between positive and negative states of electricity.

Whiston, whose works on subjects of natural philosophy remain good to the present day, and who in his own time was esteemed as one of the most accomplished of teachers, must have influenced the young student most favourably by charging his mind with lines of thought which would make him more than the mere student of physic intent only on medical details; whilst under the truly distinguished Cheselden he would be grounded in anatomical and surgical knowledge and skill in a manner simply incomparable at the date in which he was an aspirant for medical and scientific fame. That Monro availed himself freely of these advantages, and from the first was ambitious to excel, is shown by the position he took amongst his compeers. There existed in London amongst those compeers a society for the cultivation of scientific medicine, the name and precise constitution of

which is now lost, but which exercised, as it would seem, considerable service for good amongst those who took part in its labours. Monro joined this society, and read before it a paper on Anatomy, which formed afterwards the basis of his first anatomical essay. He also became a skilful and industrious dissector, and having made some dissections of more than ordinary excellency he packed them off to Edinburgh, in order that Pater Monro might see with admiring, and let us hope critical, eyes the practical advancement which his son had attained. Pater Monro was more than pleased; he was so delighted that he had the specimens laid before the Royal College of Physicians of his city for their admiring criticism, and, in time, had them mounted in a cabinet, and preserved in the museum at the Surgeons' Hall, where they remained, I have been told, for nearly a hundred years. The effect they had immediately was important enough to the sender. They led Mr. Adam Drummond, the professor and demonstrator to the college or hall of the Surgeons, to request Pater Monro to write to his son and invite him, if he continued to progress as he had begun, to return to Edinburgh, and take up his, Mr. Drummond's, chair.

The arrangement was precisely that which could most be wished for; but further study was thought to be desirable, and so our incipient professor soon set off on a journey to Paris to the famous School of Anatomy there. In Paris he attended the hospitals and the various courses of lectures pertaining to medicine and surgery, continuing his studies until the autumn of 1718, when he moved to Leyden in order to become one of the elect of the illustrious Boerhaave, the then commanding star of medical professors, and now in the full blaze of his glory. To Boerhaave he carried the best of all recommendations, a steady stream of patients, coming from Scotland to consult

the master by the introduction of a pupil, who obtained all the good wishes of the master, and whose assiduity and perseverance were subjects of general admiration.

In the year 1719 Alexander Monro returned to Edinburgh full of life and vigour, a young man of greatest promise, a young man placed in a most favourable position in the social ranks of medicine in a famous old city; and, again, a young man who through the devotion of an earnest, devoted, and trusting father had received advantages which had probably never before befallen any aspirant for medical honours in the metropolis of the North. The stars in their courses favoured this youth. He was the man wanted to found a school in Edinburgh; a school which should be a fair rival of any other in the world, and of which Paris and Leyden themselves need not be ashamed. This was his destiny, and he played his part to perfection.

At the period named, 1719, there was no regular medical school in Edinburgh. In the Company of Surgeons of that city courses of lectures were delivered on anatomy, with demonstrations, by Drummond and Macgill, who acted as teachers, after the manner of the London anatomists. Macgill was a man of considerable mark, as may be inferred from the manner in which Cheselden was led to address him; in 1719 he and Drummond were the conjoint professors in the Anatomical School at the College or Company of Surgeons, and, by a fortunate chance for young Monro, they offered to resign their office in his favour. He thereupon became a member of the Company of Surgeons, and took upon himself the duties of the professorship which they vacated. Soon after this, at the instance of his father, he undertook also to deliver a public extra-academical course of anatomy. The older gentleman, perfectly confident in the ability of his

Alexander to meet any difficulty, and to accomplish any task he might lay his hand to, invited, without the knowledge of his son, the President and Fellows of the College of Physicians and the whole of the Surgeons to attend and listen to the introductory lecture. It was a risky experiment, and was rendered still more risky than it otherwise might have been by the circumstance that the tyro Professor, who had prepared his lecture with the utmost care on paper, had left his manuscript at home. When, therefore, he entered the presence of so many grave and reverend seigneurs he stood lecture-struck, and unable to utter a word. By good fortune, if he had no paper notes before him from which to descant, he had plenty of notes from nature in the form of anatomical preparations; and, soon regaining his presence of mind, he began to use the natural notes freely. These had been prepared by himself, and had been brought by him from abroad; they were a novelty to his audience, and they afforded him ample themes for the exercise of a power of verbal exposition which he did not before know that he possessed. The result was a decided success, bringing down the applause of the audience, and disclosing to him his power of teaching from simple demonstration, a plan he never afterwards departed from. From that time onwards he taught always from memory, and laid down the doctrine that a professor who thoroughly knows his subject will invariably find words in which to explain it to others without any necessity for referring to notes for guidance or assistance.

The public lectures thus commenced became popular, and were backed up by another course on Botany and Materia Medica, delivered by Professor Alston, M.D., who was in fact a Professor in the University. There had been a Chair of Botany established in the University for a long time before the period named, but the occupants of the chair

appear to have carried out the duties of their office in a perfunctory manner altogether, so that, practically, the chair was a dead letter until Alston began to teach. It was a sort of beginning for medicine, however, in the University, and was so far encouraging as to lead the ever-active father of the new anatomist to propose to the physicians and surgeons of Edinburgh a plan which he had long had in his mind of organising a complete medical school in their city. As yet the facilities for such an enterprise were few; there was no hospital in the place that would serve as a clinical school; the University, very great as a seat of general culture for the youth of Scotland intended for the Church, the bar, the senate, or literature, had not favoured physic, and hitherto there had arisen no bright aspirant for medical professorial fame who could be looked up to as a light of medical learning, a light to be set on a hill to attract the wayfarers of medicine into an Asclepia that should be to them a home of rest and of study of the choicest sort. But at last here was the prime chance. Here was a teacher, whose qualities none could deny, ready for the task of teaching not anatomy alone, but surgery as well as anatomy. Here, also, was another Professor, learned in botany, capable of teaching that fascinating branch of natural science, and of blending with it the practical part, arising out of it, of *Materia Medica*. Lastly, here was the man of organisation, the father of the anatomist, a man himself of note in physic, and a man of reputation and trust, ready to stir up the authorities to the belief that a complete school of medicine was wanted in Edinburgh, and that the University was the grand centre in which the school must be established.

The idea once started was taken up warmly, and what was but an idea soon became a reality. Alexander Monro was made first Professor of Anatomy; Alston

retained his professorship of Botany and Materia Medica, now a living class; Professors Sinclair, Rutherford, Innes, and Plummer were elected into different chairs; and Edinburgh had a Medical School of her own.

To complete the undertaking, to give practice to theory, a hospital was next demanded, and once again the organising powers of the elder Monro were called into play. By the efforts of this gentleman, backed by those of the Members of the Royal College of Physicians and the Board of Surgeons, a fund was raised for building an infirmary or hospital for the sick poor; a house was taken as a temporary institution; physicians and surgeons were attached to it; medicines were furnished gratuitously by the surgeon apothecaries, and the nucleus was formed for the larger design which was to follow.

The design that followed was a hospital to contain no fewer than three hundred beds for the sick, and was planned partly by the new Professor of Anatomy, Alexander Monro, and partly by an eminent architect named Adams and the newly-appointed managers. The Crown granted a charter of incorporation with the power of nominating eleven managers, with the addition of the Professor of Anatomy, who was to be a manager by virtue of his office; and soon afterwards a building committee was formed, consisting of this Professor and George Drummond, Esq., who had several times served as Lord Provost. Under their able superintendence the work went on rapidly to its completion.

It is related in Hamilton Moore's collection of *Voyages and Travels*, and quoted by Dr. Donald Monro, the second son of the Professor, who lived and practised as a physician in London, and to whom I am indebted for the details of much of this memoir, that the new infirmary was erected by the inhabitants with great spirit and

unanimity. The proprietors of many stone quarries made presents of stone, others of lime ; merchants contributed timber ; carpenters and masons were not wanting in their contributions ; the neighbouring farmers agreed to carry the materials gratis ; the journeymen masons contributed their labours for a certain quantity of hewn stones ; and as the undertaking was for the relief of the diseased, maimed, and lame poor even the day labourers would not be exempted, but agreed to work one day in the month gratis towards the erection of the building. The ladies also assisted in their way ; they appointed an assembly for the benefit of the work, which was well attended, and every one contributed bountifully.

It is difficult to find in the whole history of philanthropic effort anything more remarkable than this sympathetic outburst of energy by the whole of a community. It is a beautiful picture of universal devotion to a great work, and had it been connected with the defence of the city from visible and mortal foes, armed *cap-à-pie*, and lying before the walls of the place, it would have gone down to history as an episode fitted splendidly for the painter, the poet, the player, the historian, and the novelist. Yet it was, in point of fact, a finer and, on the whole, nobler work than any one of war or siege. It was a work of love and of loving sacrifice for all men and for all time. A man might be proud indeed to be even a day labourer in such an effort, and proud beyond measure he would have been if he could have foreseen the grand result that was to come out of his labour. The mighty medical school of Edinburgh, second to none in regard to results, lay, *in minimo*, in that man's hand. Those walls which the masons quarried for, and the building masons and carpenters erected, what genius were they not to enclose, what practical talent to send forth to the ends of

the earth! But I shall be led away if I let my admiration run, as it wishes, into the history of the medical school of Edinburgh, so I return to the first professor, Alexander Monro.

In 1721 Monro was formally inducted into the Chair of Anatomy, and in 1725 he was formally inducted into the University. In this same year, 1725, he married Isabella Macdonald, daughter of Sir Donald Macdonald, of Macdonald, in the Isle of Skye, Baronet, by whom he had eight children. Four of these died in infancy, and four survived him, namely, John, who became an advocate; Donald, Physician to the army, and afterwards Physician to St. George's Hospital, London; Alexander, who succeeded him as Professor (commonly known as "Monro Secundus"); and one daughter, who married James Philp, Judge of the Court of Admiralty for Scotland.

The lectures of the new Professor included surgery, with anatomy, during the winter session, from November to May, and in addition he gave what we in these days should call clinical lectures in surgery. Dr. Rutherford, after 1748, gave similar lectures in medicine.

From what we have now seen of the life of Professor Monro, we may justly claim for him that he laid the foundation-stone of the school of medicine of Edinburgh. It is not going too far to say that, really, he laid the foundation-stone of true scientific and methodical teaching of medicine in this country altogether. There had been great teachers of medicine before his time, and teachers, like William Harvey, who were far more original than he. Yet, as a systematic expounder, he stands first. To use a common expression, he founded a method, and, in regard to anatomy, a method which has been followed into our own time. What his teaching was like Dr. Donald Monro recites in careful detail, well worthy of being repeated in

form of abstract, as a study even for living teachers to bear in mind.

His lectures were on Anatomy and Surgery, and they lasted each year from the month of October until the following May. He began with the history of Anatomy, giving a regular account of the most remarkable anatomists from the earliest times up to his own time, mentioning their different improvements and discoveries, the dates of the times in which they were made, and the different claims of authors to the honour of particular discoveries. The minds of his pupils thus prepared, he proceeded to the study of Osteology, beginning with the bones in general, then passing to each particular bone, demonstrating its parts, structure, uses, and the diseases to which it is liable. From the study of the skeleton he moved to the muscles, the abdominal and thoracic viscera, the brain, the nerves, the blood vessels, taking the anatomy first of each part, and next the structure and diseases of each part. He illustrated the anatomy of the human body comparatively by the dissection of different animals, quadrupeds, fowls, and fishes. He considered particularly the diseases for which surgical operations were commonly undertaken, and showed his pupils, on the dead subject, the best methods of performing the different operations, with the advantages and disadvantages of each method. He showed, also, the different bandages and other appliances and instruments used in surgery, and the circumstances in which they were useful. Finally, he wound up his winter course with some general lectures on the subject of Physiology.

We wonder in this day of many lectures and many lecturers how one man, whatever his ability, could carry on such a wide field of teaching as is here presented to view. In this day probably ten teachers would be

employed to take up all that Monro embraced in one course. But at that day teaching on the subjects named was a young art; few men were ready to teach anything; and one man who could teach all fairly was sure to draw, as Monro did, students from every portion of the kingdom. Whether the plan was not, after all, the best is still a moot point. We have had in our time teachers called, disrespectfully and vulgarly, "grinders," who, possessing the true art of teaching, have supplemented successfully the feeble arts of the many, and, like Monro, have taught better than all the rest. Nor is this wonderful, for be it observed that the subjects included in his demonstrations lie all together, are all part and parcel of one great whole, and are so indivisible that to break them up into many sections of an arbitrary kind, each section in the hands of separate teachers of different powers, is less open to success than to entrust them, in their simple unity, to one truly great teacher who understands the detail and the unity with equal precision and breadth of knowledge.

The day most certainly will return when unity of teaching will become once more the order of the Academy and University. A new school will rise to which all students will flow, as they originally did to the first Monro, and which will be the grand medical school over again.

Monro laboured as professor for nearly forty years; but when we say he laboured, we need not mean anything extravagant, for with him labour was love of usefulness. Turn to his portrait, and read in the face the sweetness and splendour of the satisfied mind and heart. Not a dot of envy, not a line of jealousy, is depicted there. A child in my library picks up the portrait as it lies on a side table, and brings it to me with the question:—"Oo is the booful gempiman?" Then, being told that he was a wonderful doctor who lived many many years ago, kisses

the picture, and restores it with brightest of smiles to its place.

He resigned the anatomical chair to his son, afterwards known as *Monro Secundus*, who in some points of view was superior to his father, that is to say, was a man of greater originality, and a more advanced and, perhaps, a more accomplished writer. On resigning his chair to this able successor he did not, however, resign work. He began to deliver clinical lectures in medicine at the new hospital, on which lectures *Dr. Duncan*, one of his pupils, reported that "it is hardly possible to conceive a physician more attentive to practice, or a preceptor more anxious to communicate instructions. In the different stations of physician,"—for he had by this time been made one of the physicians,—“of lecturer, and of manager to the hospital he took every measure for inquiring into the cause of death by dissection. He personally attended the opening of every dead body; and he not only dictated to the students an accurate report of the dissection, but with nice discrimination contrasted the diseased and sound state of every organ.”

MONRO PRIMUS AS AN AUTHOR.

The writings of this representative of the *Monro* family are usually called the writings of *Monro Primus*, in order to distinguish them from those of the second of the name who succeeded him, and who was also named *Alexander*. They were collected by *Monro Secundus* in one volume and were published, with copper-plates, with a life of the author by *Dr. Donald Monro* already referred to, and with a portrait, an autotype of which is published as the frontispiece of this *ASCLEPIAD*. The works include fifty-five essays, and take a very wide range;

and yet they are clearly the results of one particular line of thought, expressing unity of thought in every page.

The first essay is on the art of injecting the bodies of animals, an art which Ruysch had brought to extraordinary perfection. Monro carried the art to a great refinement in the process of injecting the minute blood vessels and the delicate membranous surfaces. He gave up spirituous solutions for this purpose, because he had observed that spirit, by which he meant spirit of wine, caused coagulation, and by his method he employed two processes. In order to inject the finest vessels he threw in first, by the syringe, coloured oil of turpentine, and immediately afterwards pushed on a common coarse injection, which readily incorporated with the finer fluid so perfectly that when the injection was well done the sharpest eye could not discover that two sorts of injecting fluid had been made use of. It was, I suspect, by this method Dr. William Hunter succeeded so admirably in his specimens, and it is a bit of practical information that is well worthy of being remembered and acted upon even at the present day.

A most practical chapter on the method of preparing and preserving the parts of animal bodies for anatomical uses is also supplied. For preserving such soft structures as the brain, Monro immersed the parts in a solution of refined spirit and acid, in the proportion of two drachms of spirit of nitre (nitric acid) in one pound of ardent spirit, strong alcohol.

A series of essays follow, relating to osteology and to the skeleton, all full of interest, and deserving careful study. They are succeeded by a memoir on the nerves, including an argument on the still unsolved question of the cause of muscular contraction under nervous influence. This essay, however, gives way in interest to another on

the nutrition of *fœtuses*, in which the point is discussed whether the *fœtus* is nourished entirely through the umbilical cord, or whether it receives nourishment also from the amniotic fluid, by the mouth. The inference drawn is that the nutrition is by the umbilical cord, an inference sustained by a line of arguments derived from a variety of sources, and indicating not only an extended reading of natural phenomena, but a long and candid study of other men's observations as well as his own.

I pass over several other papers, including one on cases of "tumefied ovarium," a clear and excellent chapter on the duodenum, and a series of brief communications, in order to come to a report as curious to us in this day as it was in the time it was written, namely, "An Account of the Inoculation of Small-pox in Scotland," made "To the Most Worthy Dean and Learned Delegates of the Faculty of Medicine in Paris, appointed to inquire into the Advantages and Disadvantages from Inoculation of the Small-pox." In this account the author essayed to answer the following questions: (1) Has inoculation been practised in your country, and with what success? (2) Did some of the inoculated die? (3) Did some who had undergone inoculation take the natural small-pox afterwards, and at what time? (4) Do you know that other diseases have been ingrafted with the small-pox by inoculation? (5) Whether did many after inoculation labour under various diseases which seemed to be owing to the operation, and whether did this happen more frequently or seldomer than from the natural small-pox?

Monro's answers to these inquiries are most valuable. On the first question he tells us, that long before the communication of small-pox by inoculation, communication of it by contact was carried out, and that parents whose children had not had the small-pox would watch

for an opportunity of any child of their neighbours being in good mild small-pox, that they might communicate the disease to their own children by making them bed-fellows to those that had it, and by tying worsted threads, wet with the pocky matter, round their wrists. He next informs us that the practice of inoculation was first carried out in England by Mr. Charles Maitland, a surgeon, who after successfully inoculating the children of the Royal Family returned to Scotland, his native country, in 1726, and went, first amongst his relations in Aberdeenshire, where he inoculated six children of gentlemen, of whom one, labouring under hydrocephalus, died, which raised such a prejudice against the practice that it was banished from that part of the country for twenty years. In the same year, however, he inoculated four children of a noble family in the west of Scotland, who all recovered ; while a fifth child of the same family, being thought too young and too weak to undergo this operation there, took the natural small-pox and died. Soon afterwards the practice was introduced into Dumfries, a town where the natural small-pox was generally of a malignant kind. Here the practice was begun about the year 1733, and so gradually passed into other parts of Scotland. To this history Monro added a number of figures and arguments, pro and con, bearing on the value of inoculation, and then, evidently himself in doubt, ended by remarking that he "shall not draw any conclusion, but leave the determination of this affair to better judges who are appointed by public authority for this purpose." A true Scotchman, Alexander Monro.

In answer to the second question, Monro admitted that there was a mortality to the extent of one in seventy-eight amongst those in whom small-pox had been artificially excited. He compares this with Jurin's and Sceuchzer's

estimates, namely, that amongst the affected by natural small-pox in several parts of England, one in six died, while of those affected by inoculation one in fifty died. On the third question, whether those who had been inoculated for small-pox were susceptible to the disease a second time, he was fairly explicit in the negative. On the fourth question, whether other diseases were ever introduced or owing to the operation, he said some were; and on the fifth point, whether these diseases happened more frequently or seldomer after inoculation than from the natural small-pox, he added that all his correspondents seemed to agree that "there are not near so numerous or various bad consequences after inoculation as after small-pox by natural infection;" and as he himself "had been so fortunate, or perhaps timorously cautious, that not one of those whose inoculation he had advised had a dangerous symptom during the disease, nor a bad consequence from it," he was, it might be concluded, of the same opinion.

The works of Alexander Monro are brought to a close with a treatise on "*Comparative Anatomy*," composed from notes taken at his lectures by his son Alexander, and published by that gentleman in London in the year 1744, without the author's consent or knowledge. The treatise is sound and learned, and is original perhaps in regard to its title, now a household word. Many of the essays described as belonging to the published works of the author were originally communicated to a society formed in Edinburgh by professors and others, and first appeared in the volumes of "*Medical Essays and Observations*" with which that society enriched the world of physic; essays and observations which good students still read, mark, learn, and inwardly digest.

THE CLOSING CHAPTER.

The honours which were accorded to Professor Alexander Monro in his native city were not confined to her justly appreciative affection. He was crowned with honours from other countries. The Royal Society of London conferred on him its fellowship. The Royal Academy of Surgery of Paris gave to him its diploma; and, in brief, his name and fame were known all the world over. His industry led him into the common as well as the higher walks of usefulness. He was a Director of the Bank of Scotland, a Justice of the Peace, and a Commissioner of High Roads, in all of which offices he played his part as well as he did in the university and by the bedside. A sincere friend to civil liberty, he allowed no party feeling to modify his humanity; and after the battle of Preston-pans he flew to the field to give his services to wounded victors and wounded vanquished alike, doing also his best to save the unhappy Dr. Cameron, who fell on the scaffold a victim to what is called his loyalty to a discredited Prince and a ruined cause. He had, says Donald Monro, and we may accept the words, though they fall from an attached son, "great humanity and a sweetness of temper, with a singular liberality of sentiment. He was a sincere friend and an agreeable companion, an affectionate husband and a kind father, and was never more happy than when he could serve those whom he thought deserving."

Professor Alexander Monro (Primus) lived the proclaimed threescore years and ten, when, full of days and honour, he sank, after a painful illness, into his rest. He was of strong build, but had, he thought, been injured in early life by the annual "spring and

winter blood-lettings" to which he had been rigorously subjected, and which, despite his practical and important warning, were continued, as a practice, in these islands for more than half a century after his time. In 1762 he was a sufferer from influenza, then a catarrhal epidemic. His illness was attended with much pain in the mucous passages of the bladder and lower bowel, which pain continued until his death with increasing severity. No medicine, opium excepted, gave him any relief. It was discovered after death that a "corroding fungous ulcer, two fingers' breadth," extended round the whole circle of the lower bowel, "with adhesion to the upper and back part of the bladder." His sufferings were borne manfully to the end; death to him had no fear, and "he talked of his own dissolution with the same calmness and ease as if he were going to sleep."

So closed the life of the first Professor of Anatomy and Surgery of one of the greatest schools of medicine the world has ever seen; a life right worthy of the beginning of that school, and worthy of its noble and splendid career even until now.

REPORTS, PHYSIOLOGICAL AND THERAPEUTICAL, MADE TO THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FROM 1863 TO 1872.*

I.—ON THE PHYSIOLOGICAL PROPERTIES OF NITRITE OF AMYL.

READ AT SUB-SECTION D., ZOOLOGY AND BOTANY INCLUDING PHYSIOLOGY, ON FRIDAY, AUGUST 28th, 1863, AT NEWCASTLE-ON-TYNE.

[PROFESSOR GEORGE ROLLESTON, M.D., F.R.S., IN THE CHAIR.]

HE paper I have the honour to read before the sub-section to-day has relation to the physiological action of the substance known amongst chemists as the nitrite of amyl. I have been led to make a careful preliminary study of this chemical body because of its very remarkable effects on the vital functions of all classes of animals, including man. I was brought to this study in the following manner. Professor Guthrie of Edinburgh, in distilling over the the nitrite of amyl, observed that when he came near

* At the request of many readers of the ASCLEPIAD I commence in this new volume a reproduction of the Reports on Physiological and Therapeutical subjects made by me to the British Association for the Advancement of Science from 1863 to 1872. I am not sorry to be brought to this labour; for these reports, although often quoted and referred to, have never yet been collected in a systematic form. The late distinguished American, Professor B. Silliman, in a lecture entitled "A Century of Medicine and Chemistry," delivered at Yale College on September 14th, 1871, conferred on me one of the most enviable honours I ever received, by making a most faithful abstract of the reports

vapour so as to inhale it he was seized with peculiar symptoms, namely, flushing of his face, rapid action of his heart, breathlessness, such as is produced by fast running, and some disturbance of the cerebral functions, lasting several minutes. These facts became known to Mr. Morison of Edinburgh, a gentleman practising in that city as a dentist, and very much esteemed for

up to that time, an abstract which has become a common source of reference. But as it is not complete, and is now, I believe, out of print, the present reproduction may be worth the labour bestowed on it.

What merit there may be in these reports lies in two facts :—

(1) That they introduced into medicine some powerful therapeutical agents since much used by the profession at large.

(2) That, constructed on a method of experimental research which studies the physiological action of a chemical substance by its chemical constitution, they gave a great impulse to that method, and helped to lead the way to what is now, with many new refinements and improvements, acknowledged to be the most certain direction towards sound and lasting progress.

In the present series the original matter of the reports will be followed, but some little differences of arrangement will be introduced. Thus, in place of repeating the narrative of each year as it originally stood, I shall select from the whole series of reports each particular subject reported upon, and shall bring all that is said upon it under one head. Secondly, I shall omit all recapitulations; and, in order to economise space, shall make abstracts when they will suffice. Thirdly, I shall fill up any serious omissions. Lastly, I shall add, in brackets to distinguish it as new matter, an occasional comment bearing on the application of the researches to past and present practice.

The series opens with the original report on the Nitrite of Amyl, read in 1863, a report which was at first received with such sceptical indifference that, although it was an essay of demonstration, it was not honoured with a place in the Transactions of the Association, and might have been lost altogether if the *Medical Times and Gazette* of September 26th, 1863, had not reported it. The incident, rendered memorable now by the success of nitrite of amyl as a valuable remedy in some of the most painful diseases, is a good lesson to all earnest students, teaching them to subdue too ardent an enthusiasm in regard to their best efforts, while they hold by what is true, with tenacity even in the face of despair.

his love of scientific and antiquarian researches. Mr. Morison having obtained a few drachms of the nitrite of amyl, brought the specimen to the Council of the College of Dentists in London, of which institution he was a member. His account of its effects made to the Council of the College of Dentists interested them greatly, but as they were not professed physiologists they naturally turned to me, as the lecturer on physiology at their school, and asked me to conduct an inquiry into the matter. I very willingly accepted the duty, and began my work with the specimen of the nitrite which Mr. Morison had left with the Council. When that was used up I made some more in my own laboratory, and afterwards all further supplies have been ably carried out for me by Mr. Robbins, chemist, of Oxford Street, London. The specimen which I have now placed on the table has been supplied by Mr. Robbins, and is an excellent product.

MODE OF MANUFACTURE AND PHYSICAL PROPERTIES OF THE NITRITE OF AMYL.

Nitrite of amyl is prepared by driving nitrous vapours through pure amylic alcohol after the manner shown in a rough sketch which is being passed round. The amylic alcohol is heated before the nitrous vapour is driven into it. After the action is complete the fluid in the retort is distilled, and the product which comes over under 200° Fahr. is the nitrite. It is a fluid of an amber colour, having a flavour and odour of over-ripe pears. It has a specific gravity of .913 in the specimen before us, and it begins to boil at 182° Fahr. It is soluble in equal parts either of chloroform, of pure ethylic or common alcohol, or of ethylic ether; a fact to be borne in mind in the course of physiological experi-

ments, because the substance can be rendered available for administration in these fluids used as menstruums. The mixtures do not, however, keep long after they are made, so that it is advisable to make them fresh as they may be required. The chemical composition of nitrite of amyl is $C_5H_{11}NO_2$. Diffused through the air in a chamber or jar, the vapour of nitrite of amyl extinguishes the flame of a burning taper thrust into it, unless the vapour be largely diluted with air, or unless the flame be introduced very slowly. When the vapour is well diluted with air, and the flame is brought into the jar slowly, the vapour sometimes explodes with a sharp puff. Placed so as to diffuse in a closed bottle containing phosphorus, nitrite of amyl delays the oxidation of the phosphorus. Placed in a closed jar with dead animal or vegetable substances, it acts like ammonia, ether, chloroform, or common alcohol, in preventing decomposition. As an antiseptic it is like ammonia. It destroys the colours of newly cut flowers, turning the vegetable reds into brown. It also changes the red colour of red muscular substance, making it first a palish white, and then a dirty brown.

ACTION ON THE VITAL FUNCTIONS OF ANIMAL BODIES.

In my first experiments with nitrite of amyl I inhaled the vapour of it myself. I made a small funnel of bibulous paper, and covering that on the outside with thick paper, to prevent evaporation from the outer surface, I dropped five minims of the nitrite into the funnel, and then putting the mouth of the funnel to my nostrils, gently drew the vapour into the lungs. The effects were exceedingly remarkable. After a few inhalations I found that my circulation was going at a great pace, the course of blood through the brain seeming to

be much oppressed, so that I was inclined, as if by force, to stamp upon the ground, but without any giddiness or any feeling of exaltation, like that which is induced by the inhalation of chloroform or ether. I felt also a warm glow on my face, and on going to the mirror there was no occasion for wonder, for the skin of my face was of a brilliant red colour, as if in a state of what is called, in medicine, erythema. The same red colour also extended down the neck, but faded away on the breast. On counting the radial pulse, it was discovered to be beating at 125 per minute as against the natural beat of 72. The breathing was also quickened. Ceasing now to breathe any more of the vapour for the time, I walked about the room, and in five minutes was relieved of the more pressing symptoms, but the pulse was still beating at the rate of 99 per minute, and the face was still rather flushed. In half an hour all the effects had passed away, and no untoward symptom whatever remained behind.*

* At this point of the paper Dr. Rolleston, the President, began to smile rather incredulously, and was followed in that way by some of the audience. I therefore improvised a funnel out of a piece of blotting paper, and dropping into the funnel five minims of the nitrite, invited the President to try it for himself. "That," he said, "is only fair," and in spite of my warning to do his spiriting gently, he, still somewhat incredulous, breathed the vapour quickly as well as deeply. The effects were demonstrated so intensely that he threw the funnel on the table before him, and asked me quietly if any harm was done. He also asked the audience if his face was as red as it felt, and being informed, amidst laughter, that "it was redder," he with much candour explained the folly of too severe an incredulity in respect to the effects of refined chemical substances on the animal body. He thought the phenomena the most extraordinary he had ever experienced. Afterwards spills of paper holding a few minims of the nitrite were passed round to the audience, and the same phenomena, in a minor degree, were exhibited in several persons who inhaled the vapour.

In order to be quite sure that the effects would be the same in other persons as they were in my own person, I invited several members of the Council of the College of Dentists* to submit themselves to the vapour, and they as well as others, including Dr. Thomas Wood, a colleague here present, were subjected to it. The results were the same as I have already related; but as I became more versed in the subject of the inquiry I ventured to push the action further, and thereby learned that whilst, in the human subject, no insensibility to pain is induced by continued inhalation of the nitrite, there are other symptoms induced which are most peculiar. For instance, although the mind remains quite clear there comes on after a prolonged inhalation extreme muscular weariness and disability, as if the muscles were unable to be moved by the will; symptoms, in fact, similar to, if not identical with, those which we have been accustomed to connect with the action of Woorali poison, when that is injected into the body, or administered by the mouth. Together

* The College of Dentists, after a few years of very useful and well-intentioned work, ceased to exist, being superseded by the Odontological Society, and by the Examination in Dental Surgery, instituted by the Royal College of Surgeons. The College held its meetings in Cavendish Square, in a part of the building belonging to the old Royal Polytechnic. The school of the College, held in the same place, was attended by a good number of students, some of whom have since risen to eminence in their profession. In the school the late Mr. Hulme taught Dental Surgery; Mr., now Sir Spencer, Wells, Bart., taught Surgery; Professor Bernays taught Chemistry; Dr. Spencer Cobbold taught Natural History; I taught Anatomy and Physiology; and Mr. Perkins taught Mechanical Dentistry. We had an excellent lecture room and a very respectable museum, and if we could have held on there might, by this time, have been a great National Institution for the teaching and qualification of members of the Dental Profession, with Library, Museum, Professorships, Chartered Examining Board, and a roll of Members or Fellows, including all qualified representatives of Dental Practice.

with this voluntary failure there is also an actual muscular failure of power. The muscles become flaccid, do not respond, naturally, to stimulation, and assume the character of paralysed muscles for the time.

Effects on Warm-Blooded Animals.

After having arrived so far in the study of the effects of the nitrite on the human body when the vapour of it is inhaled by the lungs, and having found that the fluid dissolved in a small quantity of diluted alcohol would produce the same effects as those above described, if, so dissolved, it were taken by the mouth, I passed to investigate its action in a more determined form on animals inferior to man. I wished to know whether it could be made to produce anæsthesia by prolonged inhalation, whether under inhalation it would kill, and if it would what was the mode and cause of death.

The animals subjected to experimental observation under the nitrite were the dog, the cat, and the rabbit, as warm-blooded animals, and the frog as a cold blood. The results in each class of case were most uniform under the same circumstances. When a warm-blooded animal is placed in a closed chamber charged with the vapour of the nitrite of amyl, so that the vapour is administered as chloroform would be in order to induce anæsthesia, there is, in the first instance, the same kind of excitement as is witnessed in the human subject. The action of the heart is wonderfully increased, and the breathing is rendered rapid. The administration being continued, under the same conditions, the excitement subsides, and the muscles sink into a helpless state as if they were paralysed. The animal then lies prostrate, but it is not insensible to external impressions. Reflex movements are readily induced by impressions made on the surface

of the body, and on being removed during the first stage of prostration from the chamber, the indications of returning consciousness of the animal are so rapid that the idea of anæsthesia is practically excluded. A deeper stage may, however, be induced by a longer administration, in and during which there are no reflex acts under any excitation, and in and during which the whole of the body is in so complete a prostration, that it is difficult to tell if life be still existent. At this period there are no signs of coma, no stertor in the breathing, no noises in the larynx, but a state of profound collapse, accompanied with the most perfect flaccidity of all the muscular structures, including not the stronger and voluntary muscles merely, but the delicate involuntary also, like the fibres of the iris, which fibres are so relaxed that the pupils are dilated to the fullest possible degree. In this extreme collapse the sounds of the heart and the respiratory murmur may be entirely lost. To express the phenomena in one word, the animal lies in a state of "*Catalepsy*." To the human subject in the same state, from an unknown or indefinable cause, we should, as physicians, pronounce, at once, in diagnosis, "*catalepsy*."

Recovery from the collapse here described was always very slow, and was attended with signs of lassitude extending over some days; but after recovery no bad results were seen, if the animal were allowed to remain under healthy conditions.

In other experiments animals of warm blood were made to continue inhaling the vapour until they were actually dead. They sank by a continuance of the cataleptic state as if they continued asleep. They exhibited no spasmodic or convulsive movement, and cadaveric rigidity was so slowly and imperfectly developed in them that it afforded, of itself, no satisfactory proof of actual death. After

death had absolutely taken place the examination of the internal organs of the body showed darkening of the blood on both sides of the circulation, without material interference with coagulation, great congestion of the lungs, and some congestion of the brain.

Effects on Cold-blooded Animals.

The effects induced by nitrite of amyl on cold-blooded animals, frogs, were still more remarkable than those already named, and were, indeed, perhaps the most remarkable that have ever been recorded from direct experiment, in relation to prolonged suspension of life. Frogs brought into the cataleptic condition by exposure to the vapour of the nitrite remained in that condition, not for hours simply, but actually for days, then became restored to active life, and lived on as if nothing had happened. Indeed, so long as the iris remained free from contraction, it was almost impossible to tell when death had actually occurred.* In one instance three frogs, rendered cataleptic by the nitrite, were retained nine days under observation, in uncertainty whether they were alive or dead. On the last named day the odour which came from them gave faint indications of putrefaction, and on looking carefully, it was noticed that putrefactive change was commencing in the webs of the feet of the animals. They were therefore cast into the waste pail, at night; but in the morning, to my extreme surprise, they were found alive, and capable of feeble movements. They all recovered, ultimately,—the decomposed structures being thrown off as slough, leaving a healthy surface beneath.

* In the frog, contraction of the pupil to a minute point is one of the surest signs of death, whilst dilatation is, on the other hand, a good sign of continued vitality.

During this period of collapse in frogs the muscular flaccidity is such that the limbs are almost as lithe as water, feeling like saturated seaweed or soft jelly, and remaining just as they are left, without a sign of movement. As I have been watching these strange phenomena my mind has often been carried back to those records of the past ages in which stories, that have been considered fabulous, have been told of persons who, put under the influence of death-like producing drugs, have recovered full vitality. Theophrastus, (Paracelsus) has spoken of a poison which taken one day would produce its effects in some future day; and this statement, long considered mythical, has within the present year been shown by Dr. Letheby to be quite possible, he having discovered a poison which could really produce this phenomenon. In nitrite of amyl we have another substance, which brings back to the mind another series of singular records, namely, the stories of the ages in which it was believed that some drugs had the power of producing a prolonged and deep sleep, resembling death itself so closely that death could not be distinguished from life. The story of Friar Laurence giving the potent draught to Juliet was based on this old fiction, or may we not now say fact, for nitrite of amyl is more potent than the Friar's potion, which lasted in effect for hours only, not days, and which depended for its potency probably on the wine of mandragora, to which, singularly enough, the nitrite itself is somewhat allied in composition.

MODE OF ACTION OF NITRITE OF AMYL.

I am now brought to the important question, in a physiological point of view, of the mode of action of the nitrite of amyl. And, firstly, does it act, primarily, through the blood upon the nervous system, or does it act

directly on the nervous system itself? For many years past we have imagined that every poison, so called, that could be absorbed by the body, is first carried round the circulation by the blood, and is, by this means, brought into contact with the nervous organization. Therefore, all action must be primarily through the blood. I have taught this doctrine, until quite lately, in my lectures on forensic medicine; but not long since it received a correction by an observation which seemed to me to prove, distinctly, the fact that there may be direct influence, of volatile substances at all events, by and through nervous matter. A dog under chloroform had the trachea laid open, and a tube inserted into the trachea, through which it commenced to breathe common air without chloroform. The animal began to show signs of recovery from the chloroform narcotism, when the gentleman assisting me, who had charge of the chloroform, re-applied, unthinkingly, the inhaler to the nostrils. As the windpipe was no longer in connection, either with the nostrils or the mouth of the animal, it seemed an absurd proceeding to try to administer chloroform in the usual manner from the mask; but the seeming blunder having been continued for a short time before it was perceived—our minds being directed to another point—it struck me that the animal was nevertheless fast falling back into deep anæsthesia. In consequence of this observation the administration was continued, and with as good a result as if the chloroform vapour had been drawn into the lungs themselves. This observation was so instructive it was followed up by a direct experiment, from which it was proved that it is quite possible to induce narcotism from the first, by inhalation, with the trachea open and an animal breathing the ordinary air. It was also found that hydrocyanic acid could be made to act rapidly under the same circumstances. These are instances

of direct action, it may be by direct absorption, through nervous matter, most probably through the olfactory tract; and the view I have come to in relation to the action of the nitrite of amyl is, that its action is directly through the nervous organization. I would not assume that it could not act through the blood; there can be no doubt it could if it were injected through the blood-vessels; but in the kind of action which has been described to-day, the process is, I think, by direct interference with the nervous function; that is to say, the vapour enters and is diffused through the nervous substance, and acts through the nerves upon the circulation.

The phenomena observed tell us, as it seems to me, very plainly, upon what part of the nervous system the vapour first acts. It acts primarily on the organic or sympathetic nervous system, paralyzing the controlling power of that system over the terminal vessels of the arterial circuit, and lessening resistance in the circuit at the terminal parts. In this change the arterial system of the heart itself shares, rapidly, with the rest of the arteries, whereupon, the heart passes into quickened motion, and the blood, meeting little resistance in its course, owing to the fact that the arterial tension is reduced, the tissues become injected with blood, and the cheeks and other vascular parts are flushed with blood. In this way I account for the rapid motion of the heart as a primary symptom, and for the blushing or redness of surface which succeeds. In this way, also, I account for the fulness which is felt in the head, and for the subsequent partial paralysis of the voluntary muscular system, probably from cerebral pressure.

To some extent we have already become familiar with some local phenomena of a similar kind, physiologically induced. We have had the fact demonstrated to us that

if a branch of the sympathetic nerve, going to a given part of the body, say the ear of the rabbit, be divided, the part is almost at once injected with blood, because the controlling nervous supply is cut off. In nitrite of amyl we have an agent which acts in a similar manner, but in a chemico-physical instead of a mechanical manner. We have also in it an agent which acts in a more extended manner, including in its action a field as wide as its own diffusion. The result is one which, a few months ago, could never have been dreamed of, and which nothing except accidental observation of phenomena, acutely noticed at first, and followed up by experimental inquiry, could have suggested and demonstrated. I look upon the demonstration as of value in itself as a physiological induction forced on the mind in relation to preceding research, and I look upon it as still more important for the light it promises to shed over other effects, up to this time all dark, relating to the mode of action of many medicines and poisons, the symptoms from which we are conversant with, but the modus of which we are quite ignorant of. We may perhaps even go further than this; there are some diseases which present symptoms very much like those which are brought about by the influence of nitrite of amyl. It may be, therefore, that the body by a mal-chemistry, if I may use such a term, may itself produce, in itself, chemical bodies, having physiological properties similar to this one; and that the symptoms which we place under the title of a disease may be simply due to the presence, in the body for the time, of a morbid product, morbidly manufactured in the body itself. It may be that what is called Trance or Catalepsy is produced in the mode described, and that hysteria, with the erythematous blushing which so often accompanies it, may have a similar origin.

MEDICINAL USES OF NITRITE OF AMYL.

Many persons who have witnessed my experiments have pressed on me the question :—what will be the future use and value of nitrite of amyl in the treatment of disease? I reply, that up to quite a recent date nitrite of amyl, like chloroform twenty years ago, has been nothing more than a chemical curiosity, and that no sufficient opportunity has yet been supplied for testing its uses and services in the treatment of disease. That a substance possessing such extraordinary and potent powers over vital processes as this agent possesses, is destined to play a conspicuous and leading part, no one who understands it properly, even at the present early stage of its course, can, for a moment, doubt. At the present moment, owing to the rapidity and intensity of action of the nitrite, I am not prepared, however, to recommend its use in any class of case. I wish to study it further physiologically, assured in my own mind that the true physiological action of every substance employed in the medicinal treatment of disease ought to be the foundation of its employment. The most, therefore, that I can state to-day is the indication of the place which this nitrite will hold in the *Materia Medica* of the future. It will not rank as an anæsthetic, of that I feel confident, although by its relaxing effect it may give intense relief, indirectly, to some severe kinds of pain depending on spasmodic contraction, since it is the most effective relaxant of muscular fibre yet discovered. In this service it is most likely to win its position in therapeutics, and in that direction it is now under my observation. I will report at the next meeting of the Association such further facts as I may obtain in the course of the next twelve months.

REPORT II.—READ AT SUB-SECTION D., ZOOLOGY AND BOTANY INCLUDING PHYSIOLOGY, ON FRIDAY, SEPTEMBER 16th, 1864, AT BATH.*

[EDWARD SMITH, M.D., F.R.S., IN THE CHAIR.]

The Report which I have the honour to lay before the Physiological Section to-day springs out of a paper read at the Newcastle Meeting last year, "On the Physiological Action of the Nitrite of Amyl."

EFFECT OF NITRITE OF AMYL ON DEAD ORGANIC MATTER.

Specimens of blood were drawn into open glass vessels, containing proportions of the nitrite varying from 1 to 15 per cent. in respect of the blood drawn. The blood thus charged coagulated in the usual manner and in the natural space of time; it became, however, of a dirty red colour. Set aside in the open air, serum escaped from the clot; but the upper surface of the blood, instead of soon becoming of a bright red from the absorption of oxygen, remained long dark. In proportion to the time of escape of the nitrite the blood remained free from decomposition, and the period of change in each vessel (five vessels were used) varied precisely according to the degree with which the blood, while in the fluid state, was charged with the nitrite. So long as there was distinct odour of the amyl-compound there was no change. The first sign of change, which even in the specimens containing the lowest charge was never observed before six hours, consisted in reddening of the upper surface of the clot; then softening followed, decomposition, and fluidity. In the heat of summer I found blood containing 15 per cent. of the nitrite remain

* In this Report repetitions in the original are omitted, and a few sentences are abbreviated.

unchanged for five days. The same observations were made on simple albuminous fluids, on fluids from animal cysts, and on saliva and certain other of the excretions.

We gather from these experiments that nitrite of amyl, like chloroform, alcohol, or other bodies to which reference has been made, arrests by its presence the change known as decomposition. That the nitrite itself remains undecomposed admits of ready proof, because it can be re-collected; and that it does not combine with the structures or parts of the structures which it preserves, is shown by the fact that the process of decomposition is set up only as the nitrite makes its escape by evolution.

PHYSIOLOGICAL EFFECTS ON LIVING ORGANISMS.

EFFECTS ON THE SKIN.

When nitrite of amyl is applied to the cutaneous human surface, and held in close contact with it by being placed under oiled silk or tinfoil, it produces after a brief period some injection of the vessels, and a slight tingling sensation with heat. If the skin be previously moistened with water for a long time, the effect of the nitrite is somewhat increased; but at no time is the action so rapid and marked as is that of chloroform or turpentine. To test the relative powers of the nitrite and of chloroform, I placed a pledget of bibulous paper an inch square, and saturated with nitrite, on one of my arms, and covered the paper with thin metal. On the other arm I placed a similar pledget saturated with chloroform, and covered it in the same manner. The nitrite pledget, retained on until it was dry, produced only pale redness and slight irritation: the chloroform caused great pain, so that I had some difficulty to keep it on, intense injection and redness, and some excoriation of skin.

At the same time I may observe that the nitrite is

undoubtedly absorbed by the skin. To prove this, I applied it to the skin of a frog by immersing the hinder limbs of the animal in a solution of it. In a few minutes the symptoms which markedly characterise the action of the substance, viz., violent circulatory action followed by prostration, were developed. I also applied some of the substance to my own skin, carefully retaining it in contact over a six-inch surface: during the application the pulse rose, sensation of fulness in the head followed, and other characteristic signs.

Administered by the mouth the nitrite is comparatively slow in its action, but very decisive. Administered to rabbits in doses of five, ten, fifteen, and twenty minims, and in more potent doses, its effects are striking. It admits of being readily given in tincture diluted with water. In five-minim doses it produces on these animals temporary excitement. The circulation is quickened, the breathing is quickened, the pupil is dilated, and the animal is restless; the symptoms subside in from five to ten minutes, and no harm seems to have been done. In doses of ten minims the symptoms are the same, but more marked. In doses of twenty minims, after the stage of excitement has passed away, depression follows, and continues several minutes, and there may be feeble convulsive action, but the animal recovers. There is no indication of vomiting.

In drachm doses the nitrite is often fatal to dogs, cats, and rabbits. The symptoms induced are violent action of the heart, rapid breathing, wide dilatation of the pupil, convulsions, not clonic but quick, and after an interval of a few minutes rapid collapse and paralysis of motion. The heart falls in its beats to a minimum, and the breathing may be reduced to one respiration in two or even four minutes. To appearance, in fact, the animal

is nearly dead. It lies like an animal profoundly narcotized with chloroform, but still it feels. When it is touched at any part or lightly pinched, it winces instantly if it has the power to move. The period of death is usually prolonged; and often when the animal seems so nearly dead that recovery appears hopeless, if it continues still to breathe, it throws off the nitrite, and ultimately recovers.

Administered by inhalation, the effects of the nitrite are elicited with remarkable precision and quickness, and the phenomena are amongst the most striking, perhaps are the most striking of any I have seen in all my large physiological experience.

I have now witnessed these effects on more than two hundred occasions, and have experienced them myself forty times: I can pronounce them absolute and valid phenomena, in no way dependent on mental excitement or fancied excitement. They are nevertheless developed differently in intensity in different persons, and they even slightly differ in the same person on different occasions. I will give briefly two examples.

On Mr. Kempton, a friend who has inhaled the vapour many times, the effect on the heart is so rapid that it can be detected after the first three inhalations. His pulse will rise from 72 to 105 in ten seconds, and he is conscious of pulsation in every large artery in his body. His face becomes as red as vermilion, and is not only subjectively but objectively heated.

On Dr. Gibb, after inhalation for a quarter of a minute, the pulse rises during the following quarter minute eight beats, and during the next quarter 20 beats; rising successively from 68 beats per minute to 76 and 88; the face meantime becomes greatly suffused, and giddiness is experienced. In both the gentlemen named, the pulse

comes down to the natural standard in two minutes after cessation of the inhaling process. On myself the symptoms are identical with those presented by Dr. Gibb.

Effects of an Excessive Dose.

In one instance I was so unhappy as to see the inhalation carried to the extreme of danger. An incredulous friend, seeing a bottle of the nitrite on my library mantelshelf, during a minute in which I was absent from the room, opened the bottle and commenced inhaling from the mouth. When I returned I found him walking the library still inhaling, his face and neck a bright red. In spite of all I could do he would inhale, till, as he said, he felt some effect. While I was using forcible efforts to get the bottle from him, he suddenly gave it me himself, and became speechless. I shall never forget the gallop of that man's heart. As he leaned against a table, the table vibrated and reflected visibly the pulsations. He panted for breath as one who has run to the extremity: I could not get him to move reasonably, and had the greatest difficulty in leading him into the open air. In a little time the excitement declined, and was succeeded by depression and partial loss of power; but fortunately he slowly recovered, and I do not think he was any worse for his misadventure; although, being a stout middle-aged man, I feared that during the excitement some mischief might have happened to the vessels of the brain.

In the anxiety of looking after this gentleman, I did not count minute by minute the pulsations of his heart; but the action was at one time 130 per minute, and the violence was extreme: both sounds were lost, or rather they occurred so quickly that the ear could not distinguish them, and the rapid motion communicated a peculiar synchronous tremor to the upper limbs.

My friend explained to me afterwards that his first sensation was that of burning in the face, but that he thought this arose from laughing; that the next thing he felt, and which at length alarmed him, was the hearing the pulsations of his own body very loudly and painfully. Then he felt a peculiar powerlessness which could not be described; but at no time did he lose either sensation or consciousness. I estimated, from the loss in the bottle, that this gentleman had been exposed to the vapour derived from the escape of twenty minims of the nitrite, much of which was lost by distribution in the air.

Further Observations on Suspended Animation.

As to the condition of frogs during the state of suspended animation, in six cases where the animals recovered, I made numerous observations. Examining the web of the foot, I found that there was no sign of circulation there. Laying open the thigh muscles and exposing them to continuous galvanic current as well as to the induction-current, and to shocks from the positive conductor of the friction-machine, I found no evidence of irritability. Exposing the muscles to water warmed to various degrees, from 70° to 100° Fahr., there was no evidence of irritability. The only circumstance that would lead an observer to infer that death had not actually taken place, was that the limbs remained flaccid. In cases where *rigor mortis* came on, although the animals would lie for many hours without undergoing decomposition, they never afterwards showed signs of irritability, but ultimately became flaccid and decomposed.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."—FREIND.

"THE HISTORY AND PATHOLOGY OF VACCINATION. A CRITICAL INQUIRY."

(BY EDGAR M. CROOKSHANK, M.B.)

 HIS work on the history and pathology of vaccination by Professor Crookshank is published in two volumes (H. K. Lewis), and is certainly one of the most important of all the works that have ever been written on the vaccination question since it first came into the arena of medical discussion. In the medical body it is probable that not one of a hundred practitioners is prepared to read these pages without some surprise. The analysis of the work and character of Jenner rendered in a late ASCLEPIAD startled not a few; but in that utterance, one who had lived long enough to feel that there is no new thing under the sun, was anxious to attribute to error, rather than to intention, the mistakes of a too fortunate man. Professor Crookshank has no such scruple. Up to the year 1887 he, like the majority of his medical brethren, had accepted and taught the doctrines in reference to cow pox "which

are commonly held by the profession, and are described in the text books of medicine."

In the year named above, whilst endeavouring to discover the origin of an outbreak of cow pox, it was proved to him beyond dispute that the cows had not been infected by milkers suffering from small pox; and this fact, with some others, brought him to the conclusion that the commonly accepted descriptions of the course and origin of cow pox were purely theoretical. Further observation at the National Vaccine Establishment of the Local Government Board convinced him that both official and unofficial vaccinators were completely occupied with the *technique* of vaccination, to the exclusion of any precise knowledge of the history and pathology of the disease from which their lymph stocks had been obtained. Lastly, in the library of the Royal College of Surgeons he found the original essay, in manuscript, which Jenner first sent to the Royal Society, entitled the "*Inquiry*," to which so much undue credit has for many years been blindly attributed.

The effect of this preliminary work was to lead our author to enter into an intimate study of details on the origin of the practice of vaccination. He traversed France, visiting the various schools; he returned to England, and visited Wiltshire, Gloucestershire, and Dorsetshire, in order to trace out, as far as possible, all the local facts bearing on his subject. He ransacked the great libraries, and seems to have given *carte blanche* to book collectors to provide him with works of reference; the end and object of the whole being the production of the two massive volumes now under notice.

The first volume, made up of sixteen chapters, included in four hundred and sixty-six pages, and illustrated by twenty-three plates, may be considered as purely historical,

and leaves less open for new historical research than most books of its kind.

In this volume the Life and Letters of Jenner extend from page 125 to 286, and comprise an analysis, mortal to the scientific reputation and even to the morality of that much-belauded man. The final passages are sufficient to show the character of this critical exposure.

“All that the *Inquiry* contained was known to dairymaids and farmers, with the exception of the doctrine of spurious cow pox, and certain speculative comments. All that was added experimentally to what had been previously practised, was the inoculation of horse pox from arm to arm, in imitation of arm-to-arm variolation. Up to the year 1796 Jenner had simply collected notes of a few cases of milkers and others who had had either horse pox or cow pox, and had resisted inoculation with small pox; and Fosbrooke tells us that up to this period he was not burdened with work. In the same year he made one experiment of inoculation on the human subject, and hurriedly wrote a paper, which was rejected by the Royal Society. Two years later he carried on a series of arm-to-arm inoculations, and then published the ‘*Inquiry*’ on his own account. These are the dry facts of the case.” Very dry indeed!

The second volume contains twenty-two parts or chapters, included in five hundred and ninety-one pages. It opens with the text of Jenner’s “*Inquiry*,” and gives the present and future day reader an opportunity of judging for himself of the Jennerian effort. This is followed by a series of other essays from different authors, and amongst them is introduced the valued treatise of our old friend, Mr. Robert Ceely of Aylesbury, a man who spoke always with the customary respect paid to Jenner, and who yet doubted, when he referred to the subject of cow pox,

whether the matter was properly understood ; and who, as the present writer once heard him say, was greatly perplexed by many statements that had been made respecting vaccination.

A whole number of ASCLEPIAD would not serve for a complete analysis of these volumes of Professor Crookshank. There is in them six weeks' reading of a close kind ; but fortunately they are so written that they can be taken up at any time without the sensation of a break in the narrative, since each chapter is, to a large extent, complete in itself. The work, as a whole, is one of reference to which the people as well as the profession will often turn. Already, indeed, the people have turned to it, and the so-called anti-vaccinators, with a relish of revenge which is quite dramatic to see, have literally grabbed it. To many of them the work, without doubt, affords a vindication of much that has been said against vaccination, especially on the point of the evidence adduced by the too earnest advocates of vaccination and the method of enforcing it by compulsory law on free and yet sceptical members of the community. Some will feel that this disqualifies the book in a professional point of view. It should not do so. If it be true that we of physic have really, for well-nigh a century past, been worshipping an idol of the market-place, or even of the theatre, why, the sooner we cease our worship and take down our idol the better for us altogether. We have set up the idol, and the world has lent itself to the idolatry, because we whom the world has trusted have set the example. But the world nowadays discovers idolatries on its own account ; and if we continue the idolatry it will simply take its own course, and leaving us on our knees, will march on whilst we petrify.

"ON PROVISION FOR THE INSANE POOR."

(BY D. HACK TUKE, M.D.)

WHENEVER we meet with the name of Tuke in medical literature we feel at once at home and familiar with a name which has become historical in the line of good and useful work for humanity at large; and, in the book under review, the present distinguished owner of the historical name stands out as the friend of the class of the poor of the poorest of our English community, the *insane* poor. The author here asks the question :—Whether the boarding out of pauper patients with strangers can be introduced into Yorkshire, on a sufficiently large scale to sensibly relieve asylums from the pressure which, judging from the past, will be their experience in the future? Also :—Whether it is possible to relieve asylums by paying a more liberal sum for the maintenance of harmless and incurable patients in their own homes; whether workhouses could be utilized for incurable lunatics; whether a capitation grant can be applied; and, lastly, whether it would be wiser to build two asylums instead of one, so as to provide a comparatively small establishment for recent and acute cases, and another, of a less expensive character, for the demented; and if so, should the smaller institution be on the same estate and under the same management as the larger one?

These are all practical points, affecting our national interests, which none except such learned and philanthropic scholars as Dr. Hack Tuke can properly settle; and in this work, which includes also an address read at the Leeds meeting of the British Medical Association last year, he supplies the soundest directions for the guidance of the statesman that could at the present moment be

suggested. He concludes, taking the insane of Yorkshire for illustration : (a) That the system of boarding out the insane with strangers is of a very restricted character, and that any sanguine hopes excited by what has been effected in Scotland by this plan will not be fulfilled ; (b) that the practice of paying something to the friends of pauper patients towards their maintenance, to a more liberal extent than is at present the case, should be encouraged ; (c) that workhouses under proper conditions, including separate lunatic wards and effectual supervision, should be used to the greatest possible extent ; (d) that the capitation grant, if it be retained, should be re-adjusted, so as to avoid offering a temptation to guardians to send chronic cases to asylums ; (e) that after every proper provision of the above kind has been made, 65 per cent. of the insane poor must still be sent to county and borough asylums ; and (f) that either distinct blocks should be prepared as hospitals for presumably curable cases, or a separate hospital for such cases at some distance from the asylum.

To all interested in the treatment of the insane poor, Dr. Tuke's modest and yet earnest book is a word in season, which our legislators might study in session, with hopefulness of application, without demur or delay.

"THE IRISH MEDICAL PRACTITIONER'S GUIDE."

(BY PROFESSOR R. J. KINKEAD, WITH A PREFACE BY A. H. JACOB, M.D.)

 HIS is a new and remarkable book, a book of a thousand, and one which cannot be taken up without being read from its alpha to its omega. Dr. Jacob, in his admirable and concise preface, makes the objects of the work clear, but, like a prudent

advocate, leaves the evidence to speak for itself; and it does speak without any ambiguity. Professor Kinkead deals with questions of professional responsibilities, consultations, the medical Acts, boards of guardians, dispensaries, public health, lunacy and lunatic asylums, prisons, county infirmaries, malapraxis, medical evidence, the coroner's court, poisoning, anæsthetics, abortion, food and dietaries, life assurance, and many other topics, all arranged in distinct parts, and all discussed from an original point of view in a style which carries the reader along in the readiest possible manner. The chapter on consultations is one of the best of its kind, and the author's views are deserving of the most careful attention, especially by the young practitioner. Here is a passage on the reason and intention of a consultation:—

“Where the medical attendant has a doubt as to the diagnosis, the prognosis, or the treatment, and the case is, or is likely to be, serious, he is bound to ask for a consultation. A doctor is called in, not to earn fees or to enhance his reputation, but by the exercise of his skill and knowledge to relieve pain, control disease, and save life. It is his duty to take charge of his patient ‘for the sole purpose of doing him good,’ and if he has any doubt, if the case be complicated and perplexing, if he does not see his way clearly, he plainly ought to try for light by free and full discussion with another practitioner. . . . If he differ from the consultant the statement of their want of accord should be made either to the patient or friends at the time; and, according to their wish, either a further consultation with some other practitioner held, or he should conduct the case according to his own views, or retire from its management.”

I give this extract as a taste, merely, of subject and quality, but wherever a page is opened there is something

found that deserves to be remembered. The only regret is that the *Guide* is confined to our Irish brethren. Why cannot the Irish Medical Association, by and under whose authority the *Guide* is published, extend its guidance, and empower Professor Kinhead to open his mind to all readers of our English tongue?

TO CORRESPONDENTS.

THE JELLY-FISH STING.

THE opusculum on the symptoms produced by the sting of the jelly-fish, published in ASCLEPIAD No. 23, third quarter for 1889 (pp. 242-5), has brought me many communications, one of which, from my friend Dr. Andrew Wilson, I must insert here, because it explains in his lucid style the mode by which the sting is produced.

"All *Cœlenterate* animals, whereof jelly-fishes are good examples, possess," Dr. Wilson says, "organs called *cnidæ* or 'thread-cells.' Each cell is a bag or cyst, containing fluid, and having coiled up within it a thread-like filament. The slightest pressure causes the 'thread-cell' to rupture; the 'thread' (possibly a dart) is everted, and the fluid is, of course, also emitted. I presume all naturalists admit that the thread causes a wound, which is then 'poisoned' by the fluid of the thread-cells. Under the microscope you may see a *Hydra* paralyse the water-fleas it feeds on by aid of its thread-cells.

"The *Physalia*, or 'Portuguese man-of-war,' is *facile princeps* in this matter. Its sting is very severe, and inflammatory effects last for days, or even weeks. It is perhaps the best example of an animal which stings by aid of thread-cells."

MORPHINE TOLERATION.

IN confirmation of the views expressed in the article on the morphine habit in ASCLEPIAD No. 20, Dr. Merrill sends me the history of a case which has been under his observation for eight years, with every possible chance afforded to him to study the case in all its details, and to verify every statement. The history is as follows:—

"Mrs. X., widow, aged sixty, has been using morphine for seventeen years. She began its use at the age of forty-three, on account of insomnia, and has gradually increased the dose up to seven grains per day. During the past year the dose has remained stationary. Notwithstanding her age, her complexion is fresh, her step firm, her carriage erect, her appetite good, and the functions of stomach and bowels perfectly normal. She enjoys every night seven or eight hours of dreamless and refreshing sleep.

"Although possessed of considerable property, she from choice is constantly engaged in active labour, and, indeed, accomplishes an almost incredible amount of work without feeling exhausted. Careful observation, aided by long personal acquaintance, has failed to show any sign of that moral degeneration so strenuously insisted upon by various writers. She is especially devoted to charitable work, and is zealous in regard to religious matters. Her affection for her children and grandchildren is intense, and sacrifices are freely made on their account. In her demeanour she is calm and dignified, and presents no sign of irritability or other mental deterioration. As in a case which you have referred to, she is perfectly

satisfied with her present condition, and feels no necessity for reformation. Her general health is better than ever before, and it is not certain that anything would be gained by abandoning the habit. On the other hand, it has seemed probable that much would be lost."

INTERNATIONAL MEDICAL CONGRESS.

I HAVE been requested to announce in the ASCLEPIAD that the Tenth International Medical Congress will be opened in Berlin on Monday, August 4th, 1890, and will be closed on Saturday, August 9th.

The Congress will consist of legally qualified medical men who have inscribed themselves as Members, and have paid for their card of membership. Other men of science who interest themselves in the work of the Congress, may be admitted as Extraordinary Members.

Those who take part in the Congress shall pay a subscription of 20 Marks (£1 stg. or \$ 5) on being enrolled as Members. For this sum they will receive a copy of the Transactions, as soon as they appear.

The object of the Congress is an exclusively scientific one.

The work of the Congress will be discharged by eighteen different Sections.

On account of the different languages employed, a suitable number of Secretaries will be chosen from among the foreign Members. The Congress will assemble daily, either for a General Meeting or for the labours of the different sections, and the official language of all the sittings will be German, English, and French.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD is exclusively by the Author whose name is on the title-page.

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD should be addressed to the Author at his residence, 25, Manchester Square, London, W.

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, Paternoster Square, London, E.C.

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

February
15th,



1890.

APPENDIX OF ACKNOWLEDGMENTS.

- AITKIN.** "Animal Alkaloids." By SIR W. AITKIN, KNT., M.D., LL.D., F.R.S. London: H. K. Lewis. 1889.
- ALLAN.** "Clinical Instruction in Infectious Diseases." By J. W. ALLAN, M.D. Glasgow. 1889.
- ALLEN.** "The Unalterability of Strychnine after Death." By ALFRED H. ALLEN. Philadelphia. 1889.
- BIGELOW.** "Gynecological Electro-therapeutics." By HORATIO R. BIGELOW, M.D. London: H. K. Lewis. 1889.
- BLACKBURN—CALDECOT.** "Breton Folk." By HENRY BLACKBURN and RANDOLPH CALDECOT. London: Sampson Low & Co. 1885.
- BOWIE.** "Harvey on the Circulation of the Blood." Willis's translation, revised and edited by ALEX. BOWIE, M.D., C.M. London: G. Bell & Sons. 1889.
- BRAITHWAITE.** "Retrospect of Medicine." By JAMES BRAITHWAITE, M.D. London: Simpkin, Marshall, & Co. 1890. [The latest volume of the famous "Retrospect," sustaining excellently what has now become of itself a library of English medicine.]
- BIGGS.** "Notes on the Electro-magnet in Ophthalmology." By WILLIAM HENRY BIGGS, M.D. Sacramento, California. 1889.
- BROWNE, LENNOX.** "Tracheotomy in Chronic Disease of the Larynx." By LENNOX BROWNE, F.R.C.S. London: Anderson & Co. 1889.
- BROWNE, T. R.** "The ABC Directory for 1889." By T. R. BROWNE. London. 1889.
- BURNS.** "Temperance History." By DAWSON BURNS, D.D. Part I. 1826—1842. London: National Temperance Depôt. 1889. [By far the most faithful and learned history of the Temperance Movement extant during the period specified.]
- COLLINS.** "Spinoza the God-intoxicated Man." By W. J. COLLINS, B.Sc. Enfield. 1889. [An extremely able epitome of the lifework of the great Spinoza, with, perhaps, rather more of admiration than Spinoza himself would have liked.]
- CORFIELD.** "On the History of Sewage Disposal Enquiries." By PROFESSOR W. H. CORFIELD, M.A., M.D. London: Ash & Co. 1888.
- COTTON.** "Thorough Cultivation." By A. COTTON. London: Simpkin & Co. 1887.
- CRESPI.** "Droitwich and its Brine Baths." By A. J. H. CRESPI, M.D. Wimborne. 1889.
- CROOKSHANK.** "History and Pathology of Vaccination." By E. M. CROOKSHANK, M.B. London: H. K. Lewis. 1889.
- DAVIES, W. BOWEN.** "Llandrindod Wells as a Health Resort." By W. BOWEN DAVIES, L.R.C.P. Bristol: 1889. [An address delivered before the Shropshire and Mid Wales Branch of the British Medical Association, in which the advantages of a new and promising health resort are vividly set forth.]
- DAVIS, ERNEST A.** "What is our Duty in Reference to Alcoholic Liquors?" By ERNEST A. DAVIS. Bristol: Rose & Harris. 1889. [A powerful appeal for total abstinence.]
- DAVIS, N. S.** "Mode of Action and Value of Antipyrine in Typhoid Fever." By N. S. DAVIS, JUN., A.M., M.D. New York: Trows & Co. 1889.
- DEMARQUAY—WALLIAN.** "Essay on Medical Pneumatology." By J. N. DEMARQUAY, translated by SAM. S. WALLIAN, A.M., M.D. Philadelphia: F. A. Davis, 1889.
- DUDLEY.** "Pressure Forceps *versus* Ligature and Suture." By E. C. DUDLEY, M.D. 1888. Philadelphia: Dornem & Co. 1883. [Dr. Dudley speaks with much favour of the use of hæmostatic forceps, not only in hæmostasis but also in the closures of wounds.]
- FARRISH.** "The Legal Responsibility of Inebriates." By JOSEPH FARRISH, M.D. Philadelphia. 1889.
- FOTHERGILL.** "The Town Dweller." By J. MILNER FOTHERGILL, M.D. London: H. K. Lewis. 1889. [A posthumous work of the late J. Milner Fothergill, and strikingly characteristic of his descriptive power.]
- GRIFFITH.** "Address to the Institution of Civil Engineers of Ireland." By JOHN FORSTER GRIFFITH, M.I.C.E. Dublin: Falconer, 1887.
- HAZELL—PRICE.** "Hazell's Annual for 1890." Edited by E. D. PRICE, F.G.S. London: Hazell, Watson, & Viney, Ltd. 1890. [Year by year this most useful Annual improves and becomes more necessary than ever as a library companion. An essence of English life and manner in the current time, it will be a book as good for the future as it is for the present; a veritable mine for the historians of a more advanced generation.]

- HUSSEY.** "In Accidents and at other Times." By E. L. HUSSEY. Oxford: Horace Hart. 1889. [A lecture delivered by one of the best provincial surgeons to the Oxford companies of the Oxfordshire Rifle Volunteers. Full of useful practical information.]
- JAMES.** "The Therapeutics of Kronequelle Water." By PROSSER JAMES, M.D. London: Trübner & Co. 1889.
- JEFFRIES, JOY.** "Annual Address of the Massachusetts Medical Society." By B. JOY JEFFRIES, A.M., M.D. Boston: David Clapp & Son. 1888.
- JOHNSON.** "Essay on Asphyxia." By GEORGE JOHNSON, M.D., F.R.C.P., F.R.S. London: J. & A. Churchill. 1889.
- JONES, JOSEPH.** "The Relations of Quarantine to Commerce in the Valley of the Mississippi River." By JOSEPH JONES, M.D.
 "A New Method of Testing the Germicidal and Antiseptic Power of Certain Mineral and Vegetable Substances Employed for Wounds, Tumours, etc." By JOSEPH JONES.
 "Medical and Surgical Memoirs." Vol. III., part ii. By JOSEPH JONES, M.D. New Orleans: Published by the Author. 1889.
- JONES-SPIELMAN.** "On the Comparative Anthropometry of English Jews." London: Harrison & Sons. 1889. [A laborious research, rich in results and candid in statement to a refinement rarely met with.]
- KINKEAD-JACOB.** "The Guide for Irish Medical Practitioners." By R. J. KINKEAD, M.D., J.P. With a Preface by A. H. JACOB, M.D. Dublin: Falconer. 1889.
- KNIGHT.** "Vegetarianism in Relation to Health." By JOSEPH KNIGHT. Manchester: Heywood. 1889.
- KNOTT.** "The Fever of Over-Exertion." By J. F. KNOTT, A.B., M.B., M.R.I.A.
 "Notes of the 'Fatigue Fever' of M. Peter." By J. F. KNOTT, A.B., M.B., M.R.I.A.
 "Heredity in Purpura Hæmorrhagica." By J. F. KNOTT, A.B., M.B., M.R.I.A. Dublin. 1888.
 "The Old Venetian Bleeding-glass." By J. F. KNOTT, A.B., M.B., M.R.I.A. Dublin: Falconer. 1889. [These four essays by Dr. Knott are full as an egg of instructive reading. The essay on the "Fever of Over-Exertion" is specially valuable.]
- LACEY.** "Case for Total Abstinence." Prize Essay. By W. J. LACEY. London: National Temperance Depôt. 1889.
- LEE.** "The Precedent Cause of Rickets." By DR. ROBERT J. LEE. 1889. [Dr. Lee infers that the preceding cause which most constantly recurs in the histories of cases of rickets is some form of pulmonary disturbance, generally indicated by the term "bronchitis." He appends a table of fifty cases.]
- MACDONALD.** "Board School Laryngitis." By GREVILLE MACDONALD, M.D. London: A. P. Watt. 1889.
- MCDUGALL.** "Drink and Poverty." By COUNCILLOR ALEXANDER MCDUGALL. Manchester: United Kingdom Alliance. 1889.
- MARKHAM.** "Ancient Punishments in Northamptonshire." By C. A. MARKHAM. Northampton. 1886.
- MASON.** "The Etiology of Dipsomania." By LEWIS D. MASON, M.D. Hartford, Connecticut: Case, Lockwood, & Braward. 1889.
- MITCHELL, KATE.** "Drink Question: Its Social and Medical Aspects." By KATE MITCHELL, M.D. London: Swan Sonnenschein & Co.
- MURRELL.** "Chronic Bronchitis and its Treatment." By WILLIAM MURRELL, M.D., F.R.C.P.
 "Masso-therapeutics or Massage." By WILLIAM MURRELL, M.D., F.R.C.P. London: H. K. Lewis. 1889.
- OGLE.** "Relief of Tympanites by Puncture of the Stomach." By JOHN W. OGLE, M.A., M.D. London: J. & A. Churchill. 1888.
- PASCOE.** "London of To-Day." By CHARLES EYRE PASCOE. London: Hamilton, Adams, & Co. 1890.
- PEARCE.** "I. Alcoholic Paralysis. II. Acute Dilatation of the Heart. III. Toxic Hysterical Paralysis." By WALTER PEARCE, M.D. London: Barrett & Son. 1889.
- PHILLIPSON.** "Description with Illustrations of the Phillipson Drainage System." By BURTON R. PHILLIPSON. Dublin: Chapman. 1886.
- ROOSE.** "Leprosy and its Prevention." By ROBSON ROOSE, M.D. London: H. K. Lewis. 1890.
- SCHOFIELD.** "Manual of Hygiene." By A. T. SCHOFIELD, M.D. London: Allman & Son, Limited. 1889.

- SEWILL.** "Inaugural Address of President of the Odontological Society of Great Britain." By HENRY SEWILL, M.R.C.S., L.D.S. London: Harrison & Sons. 1889.
- SMITH, LAPHORN.** "A Year's Experience with Apostoli's Method." By A. LAPHORN SMITH, B.A., M.D. 1889.
- SMITH, W. R.** "Introductory Lecture on Forensic Medicine at King's College." By W. R. SMITH, M.D., F.R.S.E. London: 1889.
- SOLIS-COHEN.** "Therapeutics of the Gouty Diathesis." By SOLOMON SOLIS-COHEN, A.M., M.D. Philadelphia. 1889.
- "Common Membranous Sore-throat." By J. SOLIS-COHEN, M.D. New York. 1889.
- SHOEMAKER—AULDE.** "Treatise on Materia Medica, Pharmacology and Therapeutics." By JOHN V. SHOEMAKER, A.M., M.D., and JOHN AULDE, M.D. Vol I. Philadelphia and London: F. A. Davis. 1889.
- STEENBUCH.** "Nogle Undersogelser af Grundluften, Grundvandit og Jordbunden i Kjobenhavn og Frederiksberg paas Foranstaltning af Selskabut for Sundhedsplejen i Danmark." Ved C. AMBT. F. JOHNSTRUP OG CHR. STEENBUCH. Kjobenhavn: Lunds. 1888.
- STEWART.** "The Treatment of Inebriety." By JAMES STEWART, B.A. London: H. K. Lewis. 1889.
- TAYLOR.** "Grand Canary: Its Climate and Springs." By CLEASBY TAYLOR, M.D., M.R.C.S. Leicester: 1889.
- TEBB.** "The Results of Vaccination." By WILLIAM TEBB. London: R. W. Allen. 1887.
- TUCKEY.** "Psycho-therapeutics or Treatment by Sleep and Suggestion." By C. LLOYD TUCKEY, M.D. London: Baillière, Tindall, & Cox. 1889.
- TUKE.** "Provision for Insane Poor in Yorkshire." By HACK TUKE, M.D., F.R.C.P. London: J. & A. Churchill. 1889.
- VACHER.** "Le Dragon Rouge." By FRANCIS VACHER. Sydney and London: Griffith & Co. 1889.
- VIVANT.** "Les Maladies Epidémiques dans le Midi." Par DR. J. E. VIVANT, de Monaco. Paris: Octave Doin. 1889.
- WALLACE.** "Vaccination proved Useless and Dangerous." By A. R. WALLACE, LL.D. London: E. W. Allen. 1889.
- WILLARD.** "Osteotomy for Anterior Curvature of the Leg." By DE FOREST WILLARD, M.D., PH.D. Boston: Kellogg. 1889.
- ZOUCHÉ (De).** "On Ebriety and the Duty of the State with Regard to Inebriates." By ISAIAH DE ZOUCHÉ, M.D. Dunedin: Braithwaite. 1885.

MISCELLANEOUS.

- "Chemist's and Druggist's Diary. London: 1890.
- "Home Rule and Federation." By A DOCTOR OF MEDICINE. London: Truelove. 1889.
- "Milk Scarlatina." "Pasteur and the Pasteur Institute." (Reprints from the Sanitary Annals.) London: Williams & Norgate. 1889.
- "Weiss's Illustrated Catalogue and Price List." Compiled by J. F. FOVEAUX. London. 1889.
- "Wood's Medical and Surgical Monographs." Vol. III. New York: W. Wood & Co. 1889.

EXCHANGES.

"The New York Medical Journal." "Journal of Chemical Society." "Journal of Society of Arts." "Chicago Med. Journal and Examiner." "New Orleans Med. and Surg. Journal." "Sacramento Med. Times." "Illustrated Science Monthly." "New England Med. Monthly." "Chemical News." "Clergyman's Magazine." "College and Clinical Record." "Glasgow Med. Journal." "Bristol Med. Chir. Journal." "Australasian Med. Gazette." "Journal of Mental Science." "Quarterly Journal of Inebriety." "Sanitary Journal." "Hygiène Pratique." "Med. Temperance Journal." "The Polyclinic." "Asylum Journal." "Journal of Microscopy." "Longmans' Magazine." "Gentleman's Magazine." "Belgravia." "Chemist and Druggist." "Health." "Sanitary World." "Journal d'Hygiène." "Malthusian." "Animal World." "Scientific American." "Bollettina della Società Fiorentina d'Igiene." "Med. Record." "Bulletin de l'Académie de Médecine." "Liverpool Med. Chir. Journal." "Medico-legal Journal." "Hospital Gazette." "Journal Scottish Meteorological Society." "International Journal of the Med. Sciences." "Journal of Heredity." "The Health Journal." "Science Gossip." "The Med. World." "The Photographic News." "Edinburgh Med. Journal." "The Wayfarer." "South African Med. Journal." "Phrenological Magazine." "Med.

and Professional Review." "Vegetarian Messenger." "The Analyst." "The China Med. Missionary Journal." Shanghai. "Séances et Travaux de l'Académie des Sciences." No. 5. "The St. Louis Med. and Surg. Journal." "The Journal." "The Canada Health Journal." "The Draper's Record." "The School Board Chronicle." "Our Day." "The Scientific Inquirer." "Public Health." "The Satellite." "The Band of Hope Journal." "University Medical Magazine." "Illustrated Medical News." "The Journal of Physiology." "New York Abstract." "Murray's Magazine." "Journal of the American Medical Association." "The London Figaro." "The Atlantic Monthly." "Provincial Medical Journal." "Albany Medical Annals." "Occidental Medical Times." "Diet and Hygiene." "The Open Court." "Journal of Physiology." "Journal of Nervous and Mental Disease." "Pictorial World." "Horticultural Times." "Illustrated Missionary News." "Vaccination Inquirer." "Medical Analectic and Epitome." "Journal of Dermatology." "The Photographic Review." "The Medical Record." "Good Health." "Medical News." "Journal of British Dental Association." "American Journal of Medical Sciences." "Drug Topics." "Journal of British and Foreign Health Resorts." "Abkari, Indian Temperance Journal." "Woman." "The Central Review." "Rivista Internazionale d'Igiene, No. 1, Abstract and Index." "Rivista de Ciencias Medicas," No. 9. 1889.

REPORTS AND TRANSACTIONS.

"Proceedings of the Society for the Study of Inebriety." London: 1889. "Proceedings of the Physiological Society." London: 1889. "Transactions and Proceedings of the American Philosophical Society." 1889. "Supplementary Register to Transactions of the Royal Academy of Medicine of Ireland." Vol. VI. 1888. "American Philosophical Society." By H. PHILLIPS, JUN., 1889. "The Monthly Bulletin of the Iowa State Board of Health." 1889. "Transactions of the Odontological Society of Great Britain." London: 1889. "Saint Bartholomew's Hospital and College." Sessions 1889-90. "Sanitary Chronicles of the Parish of Marylebone." London: 1889. "Bureau of Education, Circular of Information No. 5," 1888. Washington: 1888. "Monthly Bulletin of the New York State Board of Health" for 1888, and February and May 1889. "Programme of Arrangements of the Worcester Meeting of the Sanitary Institute," held October 1889. "Seventh Annual Report of West London Medico-Chirurgical Society." London: 1889. "Report of City of London and Finsbury Band of Hope Union." London: 1889. "Seventy-sixth Report of the Royal Edinburgh Asylum for the Insane." Morningside: 1888. "Eighteenth Annual Report of the Free Public Libraries Committee of the Borough of Leicester," 1888-9. Leicester: 1889. "Report of the Council of the After-Care Association," for 1887-8. London: 1888. "Report of the Council of the Folk-Lore Society," December 1888. London: 1889. "Eleventh Annual Report of the Society for Training Teachers of the Deaf and Dumb," December, 1888. London: 1889. "One hundred and fifth Session of the London Hospital and Medical College, Mile End." 1889-90. "Report of Committee to assist the Commission of Amended Orthography," 1889. Pennsylvania: 1889. "Eighth Annual Report of the Society for promoting the Return of Women as Poor Law Guardians," June, 1889: London. "Meteorological Report for January, February, March, April, and July, 1889." "Supplementary Report of the Committee appointed to consider an International Language," read before the American Philosophical Society," December, 7th, 1888. "Annual Report of the Incorporated Society of Authors, 1889." London: 1889. "Thirteenth Report of Working Men's College, 1889." London: 1889. "Annual Report of Medical Officers of Health, February 1889, by Authority of River Tyne Port," 1889. "Eighth Report of the Public Libraries Committee for 1888-9 of Newcastle-upon-Tyne." Newcastle-upon-Tyne: 1889. "Fifth Biennial Report of the Board of Health of Iowa." Des Moines: 1889. "Report of the Italian Exhibition." London: 1888. "Report of the Commissioner of Education for 1887-8." Washington: 1889. "Report of Public Analyst for Parish of St. Mary, Battersea." By C. E. CASSAL, F.C.S. "Public Analyst's Report for Parish of St. George, Hanover Square," June, 1889. London: 1889. "Annual Report of Parish of Kensington." By C. E. CASSAL, F.T.C., F.C.S., 1889. "Sixth Annual Report of the Association of Public Sanitary Inspectors of Great Britain, October 1889." "Annual Report of Medical Officer of Health for Borough of Salford, 1888." By JOHN TATHAM. Salford: 1889. "Report of Outbreak of Typhoid Fever in Newtown and Macdonald Town." By J. ASHBURTON THOMPSON, M.D., and GEORGE H. STRAYTON, M.L.C.E. New South Wales: 1889. "Report of Outbreak at Balranald caused by Polluted Water." By J. ASHBURTON THOMPSON, M.D. (Brux.). Sydney: 1889. "Annual Report of James Murray's Royal Asylum, Perth." Perth: 1889. "Annual Report of Women's Christian Temperance Union." By Mrs. MARY H. HUNT. Boston: W. S. Best, 1888. "Reports of the Medical, Surgical, and Pathological Registers of Middlesex Hospital for 1888. London: 1889.



ASPHYXIA, APNŒA, AND SYNCOPE OF THE LESSER CIRCULATION.

EXPERIMENTAL as well as ordinary medical observation and practice have led me to a new conclusion on the use and application of the common terms *Asphyxia* and *Apnœa*.

Hitherto these words have been ordinarily employed as if they meant one and the same thing, and in a sense they do mean one and the same thing. They mean an arrest of life, commencing at the lungs. But when we come to a strict analysis of the modes of arrest of life in the lungs, we arrive at two modes, each one of which is as distinctive as any special mode can be, to one of which modes the term *asphyxia* is strictly applicable, to the other the term *apnœa*.

I had an inkling of this fact over twenty-nine years ago, in studying out my essay on Cardiac *Apnœa*. At that time I saw that there was a form of death, attended with severe pulmonary symptoms, which could not be called death by *asphyxia*; and in order to make this quite plain, I invented the term named above, Cardiac *Apnœa*, under which I defined “a form of breathlessness, often terminating in death, due to the fact that in the cases in which it is presented the pulmonic current of

blood is being withdrawn from the air." The term, in the manner I then used it, as applied purely to apnœa arising from arrest of the action of the heart, was quite correct, and the readiness with which it was accepted and almost universally adopted by the profession indicated that it was not only short and terse, but clear and satisfactory. I have no reason now to be dissatisfied with it, nor have I any desire to withdraw it; but I see, at this moment, that I connected the word apnœa with other terms which ought to be withdrawn. I divided apnœa into several varieties: "if it arose from obstruction in the larynx it was laryngeal; if from obstruction in the bronchial tubes, bronchial; if from arrest in the structure of the lungs, pneumonic; if from arrest of blood in the heart, cardiac." Undoubtedly some of these divisions were wrong. Arrest from obstruction in the larynx or bronchial passages would give rise to asphyxia, not to apnœa; and some kinds of arrest in the lung structure itself would also come under the head Asphyxia. Further, I observed that "the blood inspires and expires, and that the production of apnœa is as perfect and as obvious in effect when the blood is cut off from the air as when the air is cut off from the blood." This I know now to be wrong. When the blood is shut off from the air, that is apnœa; but when the air is shut off from the blood, that is asphyxia. In other words, when the blood cannot expire, that is apnœa; when it cannot inspire, that is asphyxia.

The correction I would now like to make is that the term Asphyxia should be invariably and exclusively applied to conditions where the current of blood from the right side of the heart to the lungs is open, but where the air is cut off from the blood, as, for instance, in drowning or hanging; whilst the term Apnœa should be invariably and exclusively applied to conditions where the course of the

air into the pulmonary vesicles is free, but where the blood is cut off from the air, as in cases of failure of the right side of the heart, obstruction of the right side, or spasm of the pulmonary artery and its minute terminations. To use another mode of definition, asphyxia is difficulty of taking in the breath, apnœa is breathlessness.

DIFFERENTIATION OF CONDITIONS.

The local conditions and differences of phenomena in the lungs, during states of asphyxia and states of apnœa, are most distinctly marked, as also are the conditions and phenomena connected with the heart in each case. In *asphyxia* the conditions run as follow: The lungs on both sides of the chest are congested with blood, and are dark in colour; if they are punctured, dark blood at first flows from them, but on free exposure to the air the blood speedily becomes of bright red arterial hue. A portion of the lungs, or the whole structure of the lungs, removed from the chest and placed in water, will sink in the water with some escape of bubbles of air, and after a time with some change of colour towards an arterial tint, due to oxygenation from the water, and attended sometimes with a tendency to rise and float in the water. If the lungs be cut into whilst they are *in situ* they are found loaded with blood throughout all their structure, the pulmonary artery being distended with blood to its finest ramifications. Sometimes exudations of blood are detectable in the lung tissue, and more than once, in asphyxia from carbonic acid and from drowning, in large warm-blooded animals, like the dog, I have seen exudation of serous and coagulable fluid into the pleural cavities. The microscopical appearances of the asphyxiated lung are very characteristic. The plexuses of the

pulmonary vascular system surrounding the air vesicles are distended with blood, as if they had been forcibly injected; the vesicular structure is compressed, and there is exudation into the connective tissue.

In the congested condition induced by true asphyxia the elastic quality of the lungs is also seriously imperilled, especially in aged lung structure. The elastic quality of the pleural membrane is reduced, as if it had been minutely rent by the distention, and the elastic fibres of the pulmonary tissue lose in like manner their resiliency. In young animals this destruction of resilient capacity is not so much marked, but in animals of advanced life, in which resiliency, generally, is deficient, the phenomenon is effectively shown, and is, I believe, the best explanation of the well-substantiated fact that aged animals die with great rapidity under asphyxia, whilst young animals will lie for hours in a similar state of asphyxia under some favourable conditions, and will yet recover on being placed in the open air of proper dryness and warmth. In asphyxia the heart and contained blood vary somewhat according to the manner in which the asphyxia has been brought about; but as a rule, the blood remains fluid and free from coagulation so long as the motion of the heart continues to be expended upon it; a peculiar and important fact, which in all the dissections I have made, after every mode of asphyxia, has never been traversed. The heart also remains long in motion, a fact of immense importance in accounting for the curious recoveries which have sometimes taken place from prolonged asphyxia.

Lastly, under asphyxia the voluntary muscles lose their irritability very slowly, and may be seen responsive to galvanism two hours or more after the respiration has been entirely suspended.

The conditions observable under *apnœa* are, with one or two exceptions, different altogether in character. The lungs, on both sides of the chest, are more or less bloodless. They may appear, generally, of pale pink colour; they may be pale pink, dotted, here and there, with patches of white; they may be generally pale; they may be absolutely blanched, white as milk. They are usually distended with air, so that they float readily in water, and in some instances, where the *apnœa* has been long continued, they may be emphysematous. In cases of *apnœa* from obstruction of the right side of the heart by fibrinous concretions this indication of emphysema of the lung may sometimes be detected during life, and in young children may cause the walls of the chest to be expanded into visible deformity. If the lung structure be punctured, it fails to give out blood; but if a portion of the lung be taken between the fingers, it crackles or crepitates, under pressure. The microscope shows the vascular canals more or less bloodless, and the vesicular structure distended, broken, or disintegrated; but there is no marked sign of infiltration. In the anæmic condition of the lungs incident to *apnœa* there is one state resembling what occurs in asphyxia: the pulmonary tissue has lost much of its resiliency. This is due, probably, to the rupture of tissue which takes place under the forced breathing; or it may be due to the dryness of structure connected with the withdrawal of blood. However it may be accounted for, it exists, and in the aged is a cause of speedy dissolution.

In *apnœa*, taking the signs and conditions observable after death as the guide, the heart is in quite a different state from what it is under asphyxia. The heart may present various pathological states, such as fatty degeneration of its walls, ossification of its valvular parts, atrophy of its

walls, or, on the other hand, hypertrophy. Or it may be, according to ordinary observation, natural. Under any of these states, even the last-named, it will yield evidences of deficiency of power for propulsion of blood. The circumstances to which it has been subjected will have interfered with its own nutrition and action. Examined immediately after death, in the lower animal, with all its structure free of organic change, it is discovered in some instances distended and filled with coagulated blood, in others empty of blood and flaccid, in a third set firmly contracted, as if left in spasm, so that the inner surfaces of the cavities lie in close apposition. In every instance there is this one thing in common, that the organ is practically dead. It shows no irritability to sustain any movement of blood. The right auricle may present contractions, to which the right ventricle may feebly respond; but they are abortive movements, and, as a rule, are quickly brought to a stop by galvanic stimulation and excitation of every kind, and they soon die out of themselves.

In the contrasts between acute asphyxia and acute apnœa there is usually a distinction as to the presence or the absence of the blood in the arteries. In acute asphyxia the right ventricle is able, in strong subjects, to force over to the left ventricle a small column of blood, which though not properly aerated, is nevertheless sufficient to keep up a suspended life, and speedily to become requickened if a fresh charge of air be admitted to the lungs. In apnœa there is no such saving clause. The left side of the heart is, as a rule, empty of blood, and the arteries, consequently, are also empty, the most fatal of all conditions into which the body, subjected to a sudden attack of apnœa, as in angina pectoris, can possibly be brought.

Lastly, under apnœa the voluntary muscles lose their irritability very quickly. To use a common expression, they collapse. The cutaneous surface loses its tension, and assumes the sinking and pallor of death.

In a word, taking into consideration the balance of the cardiac and the pulmonary circulations, with their respective actions and co-ordinations, asphyxia is a condition in which the lungs obstruct the course of the circulation of the blood, they themselves being full of blood; whilst apnœa is a condition in which the lungs, failing to receive the blood, are rendered bloodless. In asphyxia the pulmonic blood tide overwhelms; in apnœa it does not rise. During asphyxia the air cannot meet the blood; during apnœa the blood cannot meet the air.

DIFFERENTIATION OF SYMPTOMS, SUBJECTIVE AND OBJECTIVE.

The symptoms characterizing asphyxia and apnœa, whether they be subjective or objective symptoms, are quite as distinctive as are the conditions of the lungs and heart.

The first subjective symptom of asphyxia is simply a difficulty of drawing in the breath. All the muscles which are employed in inspiration may be called into play with the full energy of the sufferer, and yet fail to satisfy the natural demand. The efforts may be spasmodic and struggling in character, and yet they do not satisfy. The sufferer tells you he chokes and is fighting for his breath. The second subjective sign is that of passing into unconsciousness, or, as it has been described to me by a sufferer who recovered, into a kind of crowded dream, and then all sensation is over.

Objectively, the signs are purely those of suffocation. The breathing is laboured, the face is dark, the veins are full, the heart is labouring; but even when unconscious-

ness has set in the radial pulse may be felt, and that for a considerable time. A well-known gaol surgeon whom I knew some years ago, and who had attended at their deaths on the scaffold a large number of men condemned to die, told me that when death from the gallows was from asphyxia he had often felt the radial pulse beating for fifteen minutes after apparent death, a fact I can corroborate from what I have witnessed of cardiac pulsation after death, by asphyxia, in the lower animals. The passage into insensibility during asphyxia is attended in all warm-bloods by convulsions of the tonic character. I believe there is no exception to this rule. The convulsion is painless overaction, and of itself is not necessarily fatal—no more necessarily fatal than the convulsion of epilepsy, which is, in fact, of the same type, and is most likely largely dependent, as Marshall Hall taught, on asphyxia. After a time, the convulsions having ceased, the muscles relax, the body cools, and cadaveric rigidity is long deferred, unless the temperature of the air is unusually high. In death the darkened surfaces of the body not unfrequently become of red colour, thereby conveying to the unlearned that signs of vitality still remain.

The above is the picture of acute asphyxia. It varies somewhat, according to the cause by which it is induced. The symptoms are very sharp and determinate when drowning is the cause, for then water is drawn into the bronchial tubes, and the lungs, rendered quickly immobile, interfere with the action of the heart and bring the circulation to a standstill. The same I found to follow after asphyxia in carbonic acid gas, and in both cases, although the irritability of the voluntary muscles long remains, or may long remain, the death is most determinate, owing to the absolute death of the heart. With other modes of asphyxia, as by the introduction of gases

or vapours into the lungs by inhalation, there may be extreme pulmonary congestion, and yet the heart, if it be healthy, may retain its irritability, and may regain its functional activity under artificial respiration with pure atmospheric air. This often occurs under asphyxia from the inhalation of chloroform, more certainly under the action of nitrous oxide gas or vapour of sulphuric ether.

The first subjective sign of apnœa is not a difficulty of drawing in the breath, but of breathlessness. The entrance of air into the lungs may be entirely free, and the air may be drawn in with the least effort; but, as an unhappy sufferer from apnœa once said to me, with the most painful truthfulness, "it does no good." This is really the fact; the patient pants and inspires quickly, and yet sinks. The failure complained of, and severely felt, is not of a struggle against suffocation, but of collapse, faintness, deliquium. There is here no passing into a dreamy unconsciousness, but an exhaustion, with full consciousness up to the last. Connected with this condition there may or may not be pain of an acute kind. Often there is pain, and when it assumes its acutest form we call it angina. The fatal collapse is, commonly, sudden at last.

The objective symptoms of true apnœa are most characteristic. The patient is failing to breathe, with open air-passages. The anxiety is terrible, but there is no obstruction. It is pure and simple breathlessness, like that which is seen in healthy persons who have been running fast, or climbing a long staircase with too much rapidity. The features are drawn and anxious, the complexion is pale, and except that the lips may be slightly livid, the aspect is that of pallor and collapse. The body altogether becomes cold, and the circulation feeble. Sleep and unconsciousness are withheld; or if sleep come on, it is attended with a quick start and an awakening with

alarm on the part of the sufferer, as if in dread that he would never breathe again.

It may be said that the symptoms here described are those of syncope, or are so mixed up with those of syncope as to be indistinguishable from them. It is true that they do resemble syncope, and I mention the fact at once, in order to present a distinction which has not, I think, been stated by any one previously, but which conveys a precise definition of apnœa, what it is and what it should always as a term describe. APNŒA IS SYNCOPE OF THE LESSER CIRCULATION. It is death by *syncope commencing at the lungs*. Or, it might be practically designated *pulmonary syncope*. It differs from ordinary or general systemic syncope, in that it may be attended by partial continuation of circulation through the brain centres and other parts of the body at large, and by consciousness; whereas in general syncope the circulation being cut off suddenly from those centres and other parts, insensibility and collapse are immediate, although they may be transitory phenomena.

APNŒAL ASPHYXIA.

The broad distinctions of asphyxia and of apnœa have been described in the preceding pages. There are, however, certain states in which asphyxia and apnœa are blended, and in which death may take place under so perfect a combination of the two that it becomes difficult to distinguish, either by the symptoms or by the morbid appearances after death, to which of the two conditions death may have been most attributable. Death in the last stages of pulmonary consumption, a death often very prolonged and painful, is commonly due to asphyxia combined with apnœa. Here a prolonged asthenia produces such feebleness of the circulation that there is equal failure

of the respiration and of the circulation. The heart and the breathing die slowly together, and the symptoms are compound throughout. Sometimes in the aged we see a similar combination, but in them generally, and it may be said fortunately, the primary failure of life commences in the respiratory tract. A cold is contracted, or a bronchial affection is set up, or occasionally a slight hæmorrhage from the lungs is developed in consequence of degenerative vascular change. Under such circumstances the respiration becomes much embarrassed, the pulmonary structures are congested, fluid is thrown out into the bronchial passages—*hydrops bronchialis*—and asphyxia is the result. This probably is natural death, death by a gentle exclusion of air, death by natural anæsthesia, euthanasia.

Under the action of different gases and vapours received into the lungs by inhalation, the conditions induced may partake either of asphyxia, of apnœa, or of a mixture of both. Under nitrous oxide gas, and under ether, the extreme symptoms are more decidedly those of asphyxia. Under chloroform there may be asphyxia, but according to my experience symptoms of apnœa have been more frequently presented, and after death by chloroform in the inferior animals the indications of apnœa have certainly been most frequently presented. All irritants, indeed, which enter the lungs in the form of gas or vapour tend to produce arrest of the circulation through the lungs, by causing spasmodic contraction of the minute pulmonary circulation, and consequent apnœa; and as by their diffusion they displace air, they cause a double danger. I have seen, in the death of an inferior animal from immersion in carbonic acid itself, a condition of lung which indicated that death was as much due to local syncope of the lungs, owing to shutting off of the

pulmonary vascular current, as from asphyxia ; and after the fatal action of all vapours of the chlorine series, including chloroform, this event often takes place. I noticed the same after inhalation of ethyl bromide ; and although inhalation of amyl nitrite in moderate quantities leads to congestion of the pulmonary structures, I have observed, after death, in warm-blooded inferior animals, caused by very rapid inhalation of this vapour, that pulmonary syncope has ensued, leaving the lungs of milk-like colourlessness, as after death by pure apnœa.

I have seen precisely the same result under the inhalation of ozone. Ozone inhaled well diluted with air may be made to produce the intensest congestion of the lungs, with effusion of fluid into the bronchial passages and of serum into the pleural cavities, ending in death by asphyxia. But inhaled in the concentrated form, ozone may cause a local syncope of the lung and rapid death by apnœa. It is the same in relation to heat. As a rule, under the effect of heat, gradually raised, the lung becomes congested, and death under such circumstances may be induced by asphyxia ; but under sudden exposure to heat a different change may follow, and the death may occur from apnœa, or local pulmonary syncope, due to sudden spasmodic contraction of the minute minor circulation. I once saw this event in the human subject in the case of a young and feeble girl, who died from being overcome with heat on entering a overheated laundry. In this case the lungs were found, at the post-mortem, absolutely bloodless. Strangely, too, the effects of cold may lead to a precisely similar order of phenomena. The tendency of cold, when it is gradually applied, is to cause a distinct congestion of the lungs, leading to asphyxia and to one of the most painless forms of death. But cold air entering suddenly into the lungs may have the most positive effect

in the opposite direction. It may lead to the sudden contraction of the minute circulation, and to death from local syncope of the lung, the perfected apnœa. In applying cold to the skin by means of ether spray we see these effects of cold visibly brought out. When the blast of ether vapour first begins to carry away the natural warmth from the body the surface of the skin affected becomes red and congested; then suddenly, after a time, if the action of the vapour be kept up, the contraction of blood-vessels may be so extreme and so rapid that the blood may be suddenly excluded from them altogether, and the part be left actually bloodless and in a state of local syncope. In this observation, of an experimental kind, there is lesson upon lesson conveyed to us. The lung structure, subjected day by day, and we may almost say hour by hour, to vicissitudes of change from heat to cold or of cold to heat, must constantly be fluctuating towards apnœa or asphyxia. So, under gradual cold, death through the lungs may be by asphyxia, whilst under sudden cold of an extreme kind it may be by local syncope and apnœa. I have seen instances of death from both these conditions in the lower animals, and more than once in the human subject.

There is one other condition in which asphyxia and apnœa combine so closely as to be united in effect. I called this condition, many years ago, one of *asphyxia, commencing in the blood*. Here the blood is so empoisoned that it loses its natural power of absorbing oxygen from the air, although that gas may be freely presented to it in the respiratory tract. The result is apnœa, with pulmonary congestion and the other objective signs of asphyxia. We may properly call this condition *apnœal asphyxia*. It is well illustrated in death from uremic poisoning, and in many other toxical conditions

in which a nitrogen or sulphur compound interrupts oxygenation by its presence in the blood.

I have placed these two conditions forward—asphyxia and apnœa—in what may be called elementary terms, in order that a vast amount of misunderstanding, arising solely from an absence of first principles of agreement relating to them, may be removed, and I wonder much that the attempt has not been made before. Harmony on the essential points, what constitutes asphyxia, what constitutes apnœa, is an absolute necessity, both in argument and in demonstration. To confound the two states is unreasonable, and can only lead to confusion worse confounded as time goes on, if there be not complete rectification of the error. A patient in the state of asphyxia dies from want of air to reach the blood; a patient in the state of apnœa dies from want of blood to reach the air. We must not differ any longer on this distinction, nor any longer confound one condition with the other. I observe that almost all the unsettled controversy about the results of the Hyderabad Commission, on the fatal action of chloroform, has arisen from the absence of the proper distinction as to mode of death. Asphyxia has not been differentiated from apnœa, but if death took place from primary failure of the respiration that failure has been called asphyxia, an utterly unsatisfactory conclusion and an imperfect application of terms.

PRACTICAL DEDUCTIONS.

If it were only to bring about a more correct nomenclature, and to arrive at a clearer understanding of phenomena hitherto presented in an involved obscurity, it would be useful to have a true and acceptable definition of the phenomena of asphyxia and of apnœa. But there is still another reason for enforcing the subject on the

attention of observers who are taking part in discovering not merely the true nature of the phenomena, but also the readiest and surest way of meeting the phenomena when they are manifested, and of finding methods for their removal. Life-saving at the most critical periods when the services of the physician or surgeon are demanded is at stake here. The practitioner who cannot diagnose the road that leads to death through the lungs can form no true estimate of what ought to be done when the emergency arises for the exercise of what should be, under a perfected knowledge, a strictly correct method of action. It will take a long line of study to learn all the facts which bear on this practical issue; yet, *à priori*, it will be evident that there must be some important difference in the art required for restoring life under certain forms of asphyxia, and in that required for the same purpose under certain forms of apnœa. Under asphyxia artificial respiration suggests itself as the one and sovereign restorative. It is so, and whenever the threatened cause of death is asphyxia, in the sense in which it has been defined above, artificial respiration, and nothing except artificial respiration, is the remedial course. All else is worse than labour thrown away. The artificial respiration cannot be commenced too soon, nor continued too long, while warmth and muscular flaccidity remain.

I have known recovery to occur in a child suffering from croup after a complete asphyxia of eleven minutes, during which there was not a sign of life; and after death by hanging there are well-established facts indicating a much longer suspension of life, with subsequent recovery. In true asphyxia everything, in fact, may be conducive to recovery. The heart may be charged with blood on both its sides, and its muscular walls retain for a long time their irritability; the column of blood from

the right to the left side of the heart may be unbroken—a matter, mechanically, of the greatest moment; the motions excited by the artificial expansion and retraction of the lungs is favourable to the motion of the blood; and the blood itself is not disposed to undergo rapid coagulation. Under such circumstances a slight push on of the blood current is often sufficient to set the whole column of blood in motion, and that motion once re-established, all is well. I have noticed that one or two independent acts of natural respiration are alone sufficient to re-start the animal fire independently, and are so effective that when once they assert themselves it is safest practice to let well alone, and to allow the natural motion to complete the work of recovery.

It is an entirely different state of affairs when the vital action is suspended at the lungs, from pure apnœa. On bloodless lungs we may carry out artificial respiration for any time, without a chance of a result. Everything now is against success. The right side of the heart is usually distended with blood, and paralyzed by the pressure of blood within it. The lungs are devoid of blood, and the column of blood between them and the heart being broken, the circuit, like a broken syphon, is incapacitated. The left side of the heart and the arteries are empty of blood, and the arteries are in contraction with all the space there may be in their cavities filled with gaseous matter capable of rendering serious resistance; whilst the lungs themselves are ready to be ruptured in their cellular structure by the least over-pressure from the artificial introduction of air. In this case we may use artificial respiration as the only reasonable chance of success, and may hope, by some fortunate accident, to catch a feeble current of blood still passing over from the right to the left side of the heart; but it is rare that anything

so fortunate happens, so rare as almost to leave hope behind. At the same time a knowledge of the conditions of the lung during true apnœa is useful in enabling us to think more clearly over new lines of research, that shall lead to better results than are at present known. Moreover, a distinctive appreciation of the differences of the two states of the lungs during which death may take place from arrest of respiratory function, suggests much as to the selection of the remedies that ought to be employed, by inhalation, for the purpose of rendering the body insensible to the pain incident to surgical procedures.

The subject here discussed has, it will be seen, its practical as well as its theoretical side. I have considered it on both these sides, and may for the moment leave it, as preparatory to some new work in the same direction, based largely on what has been said. Meanwhile I wish specially to emphasize the view that there is such a condition as local pulmonary syncope, or syncope of the lesser circulation, and that this syncope or local failure is typified by the signs of pure and simple apnœa; to which, let me add, in conclusion, that the said syncope of the lesser circulation is a very common cause of sudden death under comparatively slight shocks and strains, mental as well as physical, in persons of highly nervous temperament and of feeble heart.

THE HYPNOTIC EPIDEMIC.

WE are passing at this present moment through a mental epidemic which may be called the hypnotic, or the epidemic of hypnotism.

These mental epidemics are very similar to those that are purely physical. They often take the spreading form which constitutes them epidemical. They start from one particular centre, and follow special lines of human intercourse. They recur in regular series, appear, disappear, or nearly disappear, then reappear in full force, arrest the attention of the world, and once more subside. They seem to be modified by external conditions, and it might almost be said by season, or at all events by cycles or courses of years. In the phenomena by which they are manifested the bodily or physical phenomena combine with the spiritual or mental, both leading to certain aberrations which may most correctly be designated disease, or diseased condition, of body and of mind.

THE HYPNOTIC CONSTITUTION.

The above are striking features of an epidemical outbreak; but there is one other in this hypnotic epidemic, still more striking than all the rest, namely, that the persons in whom the phenomena of it are most easily mani-

fested are they who, by nervous constitution, are most impressionable, persons who, constitutionally, are easily overcome by external vibrations, who feel most keenly the natural shocks that flesh is heir to, and who are led to rely and trust on others more than on themselves. The people so constituted, "*Hypnotics*," in our own communities, are by no means insignificant in number, and in some parts of the world they exist in very large numbers indeed. With us they are most frequently met with amongst women, but men are not exempt. We may speak correctly of them as persons predisposed to impressional influences, and in some points of view to an enfeebled organization. I see, in Dr. Tuckey's recent and able work on "*Psycho-therapeutics*," that some authors consider them all as of hysterical cast, and it is not to be denied that many of them are strongly hysterical. They are not all of this type, and with regard to them as a class, it is most correct to say that hysteria is rather to be accepted in them as a common coincidental affection than as a cause of the impressionability to which they are subject. I mean by this that they are hysterically inclined, for the same reason that they are impressionable, the tendency to both conditions lying behind both, and being independent of both. So it happens that some of the impressionable are hysterical, whilst others are not, or, at all events, have never shown proof of the connection by the presentation of decided hysterical phenomena. What the primitive peculiarity or failure may be is not easily definable, beyond the bare statement that all members of the class have certain general attributes and qualities which distinguish them, to a considerable extent, as a class. They are of the nervous, combined with the bilious temperament. They are always sympathetic in their tendencies, commonly distrustful

of their own powers, ready at any moment to fly to others in the face of danger, and in the defence of their opinions obstinate rather than resolute. In their descriptions of events with which they have been personally connected they are apt to be contradictory when closely pressed, and yet stubborn, after all, on some particular point. They are freely communicative of what they have felt and have seen, and there is, I believe, invariably, a touch of self-romance about them which makes them paint in strong colours events connected with their personal experiences. They perhaps all share in one article of belief; they believe in ghosts, and, as a rule, they do not conceal their beliefs in this matter. Hence they listen to, and read with avidity, stories of the ghostly and marvellous kind, and are even pleased at being frightened by what they hear. In plain words, an inborn love for the mysterious is a true part of their nature.

It seems to me that we need not go far to seek the genesis of the class of man and woman-kind here depicted. We may not be able to describe the constitutional basis of their mental state, but we may, perhaps, detect, without much difficulty, its natural heredity. It is eastern in its origin, and in it lingers the old and fabled lore of ghosts, fairies, elves, wizards, and witches that was a prevailing faith, held worthy of fullest acceptance, up to days separated from these days by a very few generations. Go back, in this England, six generations only, and there is a generation in which the wisest and the best of the people were firm in their beliefs in all the arts of divination, and even in the art that would now be set down to the divination of hypnotism. A great and wise, and what is more, good judge, Sir Matthew Hale, would have had no difficulty whatever in accepting all the statements that have lately been formulated relative to the power of

“suggestion,” and the power of exceptional individuals in leading or misleading others by the influence of their will alone. The picture of witchcraft is of a craft of that character precisely, nothing more and nothing less. The beliefs in these crafts are transmitted from parent to child, partly by conversation and training, partly by the more mysterious and subtle effects of proclivity inherited from the womb. Is it strange that within the narrow limits of six generations there should yet remain many persons in whom the primitive beliefs of ages are still implanted, and in whom the physical conditions necessary for the development of the impressionable nature should actively survive? So far from this being a wonder, the real wonder is that a larger community of the same type does not exist in our time. The fact that the impressionable community is so small as it is, is a mighty proof of the advance of mental progress towards a true and comprehensive grasp and knowledge of natural law and ordinance.

I spoke in last ASCLEPIAD of the *morituri*. I explained the fact of the existence amongst us of an order of adult human beings, who are at any moment liable to death from some very slight shock of either a mental or physical character. In like manner, there are always amongst us a number of *neurotics*, who are ready at any moment to be overcome by external impressions, and to give way to commands or instructions flowing from a superior will. These are the persons who come under the influence of what was in old days called spell and witchcraft, in later days mesmerism, and in these latest days hypnotism.

CONTAGIOUS HYPNOTISM.

To the fact of the existence of this class of *neurotics* must be added another fact, namely, that the phenomena of influence evidenced in them or through them, may

become contagious, in character, by imitation. When we are told that a physician who is deeply interested in the study and practice of hypnotism begins by letting the attendants at his clinique make their first observations on persons who are strongly susceptible, we cannot, in this strangely imitative world, wonder at the occurrence of contagion of influence. We are all born imitators, and in the strictest sense everything that is learnt is a thing or act imitated. The general fact of writing with the right hand is a mere matter of imitation, and the likeness of expression that is witnessed between husband and wife who have lived long and harmoniously together is imitative in precisely the same manner. But similar natures are easily imitative, *inter se*, and so the *neurotics* are of all others of the imitative species. They run together, cling together, hold together. And in like mode, in regard to physical impressions, they are similar. Amongst them the contagion of imitation rapidly spreads. At first it may be concentrated in one locality, and may remain confined to that locality for long periods, as if it were endemic in its nature. Or it may go out, and may spread wide and far, over all the world. Ordinary epidemics may play the same *rôle*; they may be at first isolated or endemic; but, under favouring circumstances for their development, may break through the original boundaries, and become, *for a time*, universal.

It is right to say *for a time*, for hereby hangs another subject of moment. Outbursts of hypnotism, like outbursts of common epidemics, and like outbursts of fashion of dress, have their seasons of development, of full growth, and of decline; then after a period during which they sleep, for they are not dead, they rise again, and appear as active as ever. As a rule, they are, after a while, outmatched by some new influence which attracts

to itself attention, and withdraws attention from the old, and wearying, and temporarily worn-out object. For this reason, we who are mere quiet lookers-on feel certain, when one of these periods of influence is in its fullest swing, that in the course of nature the swing will die down, and the occupants of it will be off to other scenes and acts of newer caste and stronger attractability.

All reasons put together, in explanation of the fever of influence leading to this particular present epidemic of hypnotism, the occurrence of the epidemic and the noise which it has made are not in any degree surprising. The phenomena are startling in themselves, they are pretty fairly numerous, and they bear a certain measure of corroboration. To the whole must be added a somewhat fine touch of innocent colouring, subjective and objective, which is not without its effect in the way of presentation and of demonstration. Now and then the demonstration is not quite so fitly demonstrative as the rigid experimentalist would desire, but it is not to be ignored on that account. Dr. Tuckey relates an example, from the observation of a chemist of Charmes, in which the effects of a blister on an impressionable subject are recorded; the effects of a blister raised, as it was believed, by suggestion. A patient, during hypnotic sleep, had applied to her left shoulder, about 11 a.m., eight postage-stamps, and had it suggested to her that the application was a blister. She was allowed to sleep all day long, except when aroused to meals, and was kept under observation. When she retired for the night she was told to sleep until the next morning. At 8.15 the dressing which had been put on was removed, and the stamps were found *in situ*. The surrounding skin "for four or five centimetres was thickened, modified, and of a yellowish-white colour." The cuticle, however, was

not raised, and did not form a blister, but it was thickened and wrinkled, and presented the appearance usual before a complete vesication. This part of the skin was surrounded by a zone of intense redness and swelling. The patient returned to Charmes, and by 4 p.m. four or five vesicles were developed. Fifteen days later the blister was still suppurating freely. The chemist, M. Focachon, made a similar experiment on another patient, and in forty-eight hours produced a blister which followed the same course. I select this example as one that to an experimentalist like myself is unsatisfactory, from the first to last, in regard to the inferences derived from it. It seems never to have been conceived that the evidence of the so-called blistering might have been of the purest physical character. It was, apparently, never asked what was the quality of the adhesive substance on the postage-stamps which made them adhere to the skin of the patient. But in this country, and in other countries, extensive and painful irritation and vesiculation have been known to occur from the contact of the adhesive matter of postage-stamps to a moist surface of the body. I myself had once to treat a case in which the tongue, the lips, and the skin adjoining the lips, were severely irritated, from the process of moistening the stamps from the lips and tongue; and, taking all the symptoms exhibited by the patient at Charmes into consideration, I, for my part, should conclude that the symptoms were due to nothing more than to the simple action of a direct local irritant. Again, if by the word "blister" was meant the common blister made from cantharides—the ordinary Spanish fly blister—the "suggestion" failed, since cantharides bring out one or two large blebs or blisters, not vesicles, which afterwards dry up, but do not suppurate.

Thus the stamps, by their action, might have done precisely what was done, and what, according to accepted experience, they sometimes do. In a word, to have made the record above described of any value whatever, the same number of stamps, cut from the same sheet, ought to have been applied at the same time to another person, without any introduction of the elements of hypnotism or of suggestion, in order that the effects of the stamps alone might have been carefully observed and recorded.

In another sentence of the able work from which I have quoted it is said that variations of the pulse have been induced by suggestion, whilst the pulse was being taken and the pulse-strokes registered by the sphygmograph. To me, accustomed every day of my life to observe the pulse by the sphygmograph, or sphygmophone, such a statement as this almost reads like banter. I never, in all my experience, put either instrument on the pulse of any one without observing a variation. Suggestion is not wanted for the purpose of observing variation. Almost invariably the pulse rises a little in beat when the instrument is first applied, and nearly any external act that for a moment diverts the attention or changes the attitude of the mind causes a variation. For this reason no two readings of the same pulse are precisely the same, and from this self-same reason the distrust which many practitioners feel for pulse-reading arises. For my own part, after an experience of over twenty years in pulse-readings, including at the very least 6,000 readings, I could not, I am quite sure, differentiate between variations induced from what is called suggestion and those which occur without the least idea of suggestion. Differences of breathing and of positions of the body are also, alone sufficient to cause variations of the pulse.

MODERN HYPNOTISM, ANÆSTHESIA, AND SUGGESTION.

It may be inferred, from what has been said, that in the experience I have gained the phenomena of hypnotism have no real value, no real meaning, no real intention. The inference, however, would not be correct. It must be accepted that in the impressionable natures to whom reference has been made, some extremely curious and important phenomena can be induced. There need be no doubt of the impressionables, and no doubt that in opposition to them there are persons of strong and resolute will, who by their suggestive influence can overmaster feebler natures and make them subservient. The power exerted is that supreme power of one person over another which is occasionally displayed, on a gigantic scale, by particular men. Napoleon I., on his return from Elba, literally hypnotized and governed, by soldierly suggestion, not one man, but thousands; not women and weak men, but soldiers who on the field of battle would know no fear. The same kind of power has been and can at times be exercised in medical practice. Under it physical pain itself can be abolished for a time, and the will of the subject be led into obedience, and even into deferred obedience, under the influence of the commanding will.

The two grand results claimed by the supporters of hypnotism are the production of anæsthesia and the influence of suggestion. It will not be lost time to discuss these phenomena in their relation to other phenomena which are accepted as more determinately physical in their nature and more definitely comprehensible.

It must be admitted, in regard to anæsthesia, that a peculiar insensibility can, under some circumstances, be produced by mental impression, without the aid of an anæsthetic. In 1859 Dr. Atkinson, a surgeon who had

recently returned from India, brought to me a long and richly illustrated report of over fifty operations, embracing some of the most crucial kind, which he had either himself performed or had helped to perform under "mesmeric anæsthesia." I was at that time bringing out the late Dr. Snow's great posthumous work on Chloroform, and Dr. Atkinson thought that the reports he possessed might be of interest to me. They were of the greatest interest, but they did not seem to either of us to be of much promise, in practical life, in this country. He explained that the proceedings emanated from what had previously been carried out in India, in the same direction, by Dr. Esdaile, and he added that they were all performed on Hindoos. Every attempt to carry out the same process on Europeans had signally failed. Another singular fact had also come under his observation, to the effect that the success on the impressionable native population was neither constant nor universal. There were many natives who could not be brought under the influence, and there were times or seasons when it was difficult to bring the impressionable natives themselves under it. There were, on the other hand, seasons when a prevailing tendency towards the mesmeric state appeared to be present. The plates illustrating the report were cleverly drawn, by an intelligent artist, and the uniformity of the appearances exhibited was striking. The patients were all represented as in deep sleep, with their features in the most placid state, but the positions of their bodies were strangely diversified. The limbs, in some instances, were drawn up to the trunk, and in other instances were widely extended out; in some the muscles were rigid, in others entirely flaccid. We felt that the position of the body in each case was exceedingly like that seen in hysterical seizure, and Dr. Atkinson thought there was the closest analogy between the state

induced by the mesmerism and that which is observable during hysterical anæsthesia. About the fact of the production of a sufficient anæsthesia, in these Indian subjects, deep enough for the painless performance of major operations, there was no dispute; yet the practice, nevertheless, declined, because of its uncertainty, and because it began to lose its effects as time went on.

Something of a similar kind was indicated in this country at one time. At the period when general anæsthesia, by ether and chloroform, was being introduced into practice, the mesmerists, under the guidance of one of the most remarkable physicians London ever possessed, the late Dr. Elliotson, were loud in their statements regarding mesmerical anæsthesia. An energetic follower of Dr. Elliotson (Mr. Purland) assured me that he had been present at an operation for amputation of the thigh, the subject of the operation being a man, in whom the most perfect anæsthesia was induced by mesmerism. I asked Dr. Elliotson as to the correctness of this experience, and he confirmed it to the letter. But when an opportunity was opened for corroboration of such striking proof of mesmeric power on an extensive scale, no similar result was repeated. The College of Dentists formed a large committee, of which I was appointed chairman, for the purpose of testing every method known for the painless extraction of teeth. The committee met a great number of times, and teeth were extracted under different processes of anæsthesia. I cannot recall a single sitting at which Dr. Elliotson and Mr. Purland were not present, and at every sitting the freest opportunities were offered them to test their mesmeric skill. As a general fact, they declined to make any attempt; but now and then, in cases which they considered favourable, Mr. Purland tried his hand. There was never any result whatever. In one instance, in which an

electric method was being tried, Mr. Purland made some "passes," and a tooth was extracted without the knowledge of the patient. For this apparent success Mr. Purland contended that mesmerism had scored; but there was so much doubt on the point that Dr. Elliotson himself would not claim the victory, his diagnosis running with mine, that the patient fainted from the electrical shock, and that syncope was the true cause of the insensibility. I was profoundly struck, when this event was over, at an observation which Dr. Elliotson made to me as we were talking alone upon what had occurred. He had previously told me, in a friendly and sad tone, how he had been persecuted for the part he had taken in regard to mesmerism; how severely he had been attacked by the *Lancet*, and how he had been displaced from his high place in the world of medicine and of practice; and now, he added, that if the results of his early inquiries had been as negative as those which we had witnessed together he should have been as sceptical as I was myself. "You are right," he observed, "in being sceptical, but not more so than I once was in believing, because we are both simply governed by what we have witnessed. What I first witnessed from mesmerism," he continued, "was as *bonâ fide* as the effect of chloroform; but of late the same phenomena are not so obtainable; the spell is broken." I have often thought of these words, and it seems to me, at this present hour, that they are of special import. The epidemic of mesmerism in the time of Dr. Elliotson, when he was so energetic on the subject, was at its height. It declined, and with its decline the casual facts on which a fervent and acute observer had rested passed away, leaving him at last in doubt, and as if he had been deceived in that which he had seen and believed.

It is only by understanding this epidemic character of

mesmerism and of hypnotism that a reason can be assigned for their appearance and disappearance. If the phenomena were fixed, like those which occur from chloroform, methylene, or ether, they would remain amongst the definite achievements of practical science. The surgeon would never think of troubling himself with the expensive and cumbersome apparatus required for the administration of chloroform, methylene, nitrous oxide gas, or ether, if he could dispense with them, and put in their place the simple and inexpensive methods of inducing hypnotic sleep and of supplying a suggestion. Above all, no surgeon would accept the risk of administering an anæsthetic if he could get the same happy results of negating pain by a plan so safe as hypnotism. There must, therefore, be some reason why the proposed hypnotic system does not establish itself permanently in the practice of medicine, and that reason must be, either a limitation in the number of persons susceptible to the system, a limitation of the period during which the system is, so to say, workable, or a combination of these two causes, most likely the last.

Before quitting this part of the subject a word must be said respecting the different susceptibilities of different persons to pain, and to the remembrance which different persons have of pain when the pang, or supposed pang, is over. They alone who have had a long experience of the symptoms and expressional descriptions of pain can form a correct idea of the perplexities and contradictions that are manifested on the subject of pain. I remember that in my early medical life one of the most widely known mesmerists of the time was accustomed to bring before his audiences a man he called Jordan. Jordan was mesmerized, and then was made to do unconsciously whatever his master bade him,—hypnotism and suggestion. Again, whilst Jordan was hypnotized he was declared

to be dead to all pain, and a surgeon, if present, would be called to the platform and told that he might stick a needle into any part, not vital, of Jordan's unfortunate body, and that not one sign of pain would be elicited. It was so. I once saw ten needles buried up to their eyes in Jordan's muscles, buried so deeply that they could not have been withdrawn if there had not been a thread to pull at. Yet Jordan gave no sign of pain whatever. The observation would almost have converted me, but it so happened that a tailor, who looked the very picture of nervousness, put me out of belief next day by showing the same experiment on himself, and by actually passing a threaded needle through his cheek, drawing the thread after it. Later in life I found that this needle experiment was easily learned, and, if I wished, I could go at this moment to one of my learned colleagues of the Royal Society, who is also a distinguished member of the House of Commons, and see him perform the experiment of burying a needle in his muscles without winking or offering a sign of pain.

In producing local anæsthesia with ether spray these curious facts multiply backwards and forwards. I once made a part of the body of a man so dead that I was afraid of having carried the local death too far. It was incredible that the patient could feel anything in those densely frozen tissues, yet when the knife of the surgeon divided them the cries of the man were heartrending, and continued to be so until the close of the operation. I went home in despair. This man, I felt sure, would denounce my invention of ether spray to every person he met. Judge, then, my astonishment when, on the following day, he wrote me the warmest letter of thanks, conveying that from the beginning to the end of the operation he was unconscious of the slightest suffering. I called upon him, and inquired why, if he felt so little, he cried so

much. "It was sheer fright," he replied. "As soon as I saw Sir William" (Fergusson) "take up his knife I began to scream, and for the life of me I could not stop; the more I tried the worse I got, but positively I never felt a single touch of pain." In contrast with this, in another case, where a tumour had to be removed from the back of a man, after I had produced the most superficial numbness, an accident happened to the spray-tube, and not another breath of ether would come over. The surgeon not perceiving the difficulty, and anxious to commence, began his part, and to my astonishment, made a long incision, certainly four inches in length, without causing the patient the least pain, and the operation went on to its completion without a sign of suffering. When asked afterwards whether he felt anything, the patient said, "Nothing whatever;" he did not know when the operation commenced, nor when it was finished, except by the conversation which he gathered from us.

Once more, I saw a patient pass through an operation under ether spray not only without showing signs of pain, but saying at the moment that it was "quite delightful to be so entirely free from suffering of any kind;" and still that same patient the next day declared that the agony was fearful, and that to save her life she would not go through the same ordeal again.

The effects resulting from local anæsthesia are perhaps more applicable to the question in hand than those of general anæsthesia; but some facts connected with general anæsthesia are of value. I have known a person come out of the deepest anæsthetic sleep, in which every reflex has been suppressed, and during which a long and serious cutting operation has been performed, and affirm, notwithstanding, that every step of the operation had been felt, and that the pain was beyond expression. On the

other hand, I have known a patient to be apparently awake during the whole of an operation, so awake as to obey every command, and on recovering express that the period of narcotism had been an absolute oblivion.

In brief, no two persons feel pain in the same degree, no two persons give the same description or the same definition of what is called pain. Lastly, on this head, pain may kill itself. The actual severity of suffering may extinguish suffering, and the will may extinguish it.

In what is called suggestion under hypnotism certain conditions of body are present in those who are susceptible of suggestion, something akin to conditions that may be induced under narcotic vapours and under the action of certain narcotic drugs and vapours. I once made a vaporizable compound which I called hydramyle. After the inhalation of this vapour, some persons brought by it into a state of dream or unconsciousness would do anything that was requested of them, and would carry on conversations about directions, asking if that would do, and the like, without any after-recollection of what had occurred. I once saw a gentleman who had inhaled another vapour, amylene vapour, which induces a kind of somnambulism, remove his boots under suggestion, and place them, also under suggestion, on a shelf, without being in any degree conscious of what he had done when he returned to his natural state. Phenomena of a similar character were witnessed in my experiments with methylic ether, and the late distinguished Professor Polli, of Milan, observed almost the same manifestations after the administration of haschisch, by which, as he remarked, the tricks of the Old Man of the Mountain were reproduced. I take it that in these experimental observations the results of a modifi-

cation of the natural nervous atmosphere are passing under review. There is a temporary modification of the mental surface, during which impressions from the outer world are more deeply made than in the natural constitution, and under these impressions the volitional centres are called actively into response. In hysteria I have no doubt that a similar perversion of the nervous atmosphere is present; and, in the condition of susceptibility in which the phenomena of hypnotism are pronounced a similar abnormal condition exists in a comparatively permanent state. In yet other conditions the nervous atmosphere is so beclouded, so empoisoned, to use a more expressive and not less truthful phrase, that melancholy holds its own, and the will, subservient to the intensity of impressions, is rendered powerless.

HYPNOTISM ON THE PRACTICAL SIDE.

What will be the effect of the hypnotical and suggestive methods on the practical life of the world?

The answer to this question is clear, if the present experience may be tested by that of the past. By such test it may be assumed that for the temporary annihilation of pain the hypnotic method will never become a serious rival of the physical anæsthesia, local and general, produced by ether and the other volatile substances. To make the hypnotic anæsthesia universally applicable we should require to have universal change in the nervous organization, under which the brain would be rendered susceptible beyond the common susceptibility to external impressions. This would mean a transformation of mankind into a new and truly miserable mental condition, an event not likely to be developed in an age when strength of intellectual growth is the grand necessity

of the day. Limited at this moment to the few who are susceptible to it, hypnotism must become still more limited as enlightenment goes on. As a method for producing oblivion from pain it will be further limited, in application, to short seasons of special social or meteorological cast; and if it fail, the influence of suggestion will fail with it.

Bringing the mind to adjudicate on the whole matter, is it not truly a happy circumstance that the forecast relating to both hypnotism and suggestion is towards their extinction rather than their advancement? Their advancement could mean nothing but weakness, the strong leading the weaker, the weaker the weakest. In this way it never can have been ordained that the human mind should travel through the ages. With such a prospect in view, an enthusiast like Dr. Tuckey himself would hardly be satisfied. Nay, it is obvious that he sees for the phenomena he relates a necessity for two natures—one the susceptible or enfeebled, the other the commanding or the strong; a terrible division unless we wish all our health and strength to die.

But if the hypnotic phenomena are passing vanities, from them a lesson may grow, touching the education of the young, of inestimable value. Before the adult man and woman, but chiefly the mother-woman, the young child is always under the dominion of hypnotism and suggestion, and suggestion made on the young mind often goes forth as an order for life, for good or for evil. What number of uncontrollable acts, follies, passions, are due to such hypnotism and suggestion on the susceptible child few have conceived; albeit they are the foundations of the good and of the evil of our common humanity.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

SEQUELÆ OF INFLUENZA.

FTER an epidemic has passed over a community, and caused a considerable amount of anxiety and excitement, it is often difficult for the physician to estimate the extent of the evil it has caused, or of the mischiefs which it has left in its train. It produces what may be called a moral epidemic, as well as a physical, and the thoughts of men, bearing on the subject of disease, seem to be coloured by it. People visit us after the disease, in its manifest form, has disappeared, saying that they are suffering from some after-effects resulting from it. I never knew a widespread epidemic which did not leave this kind of experience, but there is always about the experience the difficulty of discerning whether the symptoms complained of stand in the relation of cause and effect, or whether they may not be mere accidental or coincidental symptoms, which might have occurred if there had been no particular epidemic whatsoever.

Since the influenza epidemic has passed away, several persons have consulted me for affections which they have considered to be sequelæ of influenza. In more than half these instances there was no connection between the symptoms presented and the preceding attack of influenza. In other examples the connection was undeniable.

Palpitation of the Heart.—A lady of delicate habit of body, unmarried, and with a tendency to anæmia, suffered during a rather sharp attack of influenza, in January last, from frequent palpitation of the heart, whenever she began to rise from her bed. She recovered in all other respects, but for several weeks had a return of the palpitation, when she made any unusual effort, or rose from a chair quickly. The palpitation was of the ordinary kind, and was entirely free from any physical indication of organic cardiac disease. By very careful attention to diet, by regular and long hours of rest, by avoidance of excitement, and by the use of syrup of superphosphate of iron and peroxide of hydrogen (see ASCLEPIAD, vol. iv., p. 140), she has recovered.

Syncopal Exhaustion and Syncope.—In another case, occurring this time in a young man generally healthy, a bad attack of influenza was followed by the recurrence, at rather short intervals, of cardiac failure almost amounting to deliquium. These seizures, which took place two or three times a day during the period of the acute attack, returned repeatedly, although with less intensity and with decreasing intensity, for several weeks. Connected with them there was a general sense of feebleness and inaptitude, so that “memory was an effort.” Here, again, no sign was offered of organic cardiac disease.

Discharge from the Ear.—A young girl, naturally healthy, who had a mild attack of influenza in common with other members of her family, was affected with earache during the period of her attack. There was no inflammatory manifestation of an outward and visible kind, but relief from the pain was obtained after a muco-purulent discharge from the external meatus of the left ear. All the other symptoms passed away, but the discharge from the ear remained, with slight deafness, for nine weeks.

Hoarseness and Loss of Voice.—A gentleman who has a fine tenor voice when he is in good health was attacked severely with influenza. On the second day of the attack, and whilst he felt “an exhaustion that was unendurable,” he was rendered worse than ever by the discovery that he “could not bring out his voice,” and that a hoarseness of a painful character had become developed. He was many days ill, and many weeks passed before he could speak clearly or sing a note without pain. His throat was much injected down to the vocal cords, and the cords themselves were extremely relaxed. Together with this, he experienced a sense of feebleness in breathing, which prevented any great vocal display, and under a slight cough fluid from the throat was expectorated which was tinged with blood. Under tannin spray for the throat, with careful regimen, this patient is now convalescent.

Bilious Symptoms.—More than one patient has suffered during the attack with what the people call bilious symptoms—nausea, vertigo, pain in the right shoulder, irregular action of the bowels, yellowness of the conjunctiva, and loaded, creamy condition of the tongue. In one of these examples this train of bilious signs has proved exceedingly troublesome to treat, and cannot be said yet to have subsided completely, although the improvement is now marked and steadily progressive.

In a few other cases some transitory symptoms, such as erythema and eczema, manifested during the attack, have recurred during convalescence; but the phenomena classified above are the most notable sequelæ that have come under my own notice. They all seem traceable to a nervous origin, to a shock of the sympathetic system, and to interference with the nervous control over the minute or peripheral circulation.

NOTES ON EPISTAXIS.

LTHOUGH I have known and treated a large number of cases of epistaxis, one that ended fatally has never occurred in my practice, but a few have been exceedingly troublesome, and have required several days of careful watching. This experience has about it no novelty, for, generally, epistaxis is so far from being fatal that little importance is attached to it, and every tyro feels that he can manage it. It may be the fact that every tyro can manage it, since the danger is not often extreme, and the art of treatment is simple; nevertheless, one or two notes, derived from long observation, may be of service.

One fact has struck me as very peculiar in regard to epistaxis, namely, that it is, comparatively, rare in those who are subject to what is called the hæmorrhagic diathesis. It is true that epistaxis may occur in persons of this diathesis, and when this is the case there may be considerable difficulty in controlling the flow of blood; but still there may be extreme fluidity of blood, with hæmorrhage from other surfaces, as from the gums, and with effusion into the cellular tissue in hæmorrhagic blotches, and yet without epistaxis. On the other hand, in persons in whom the plastic quality of the blood is good there may be frequent recurrent attacks, from which we must infer that the diseased or enfeebled condition leading to the attack is, as a rule, connected with the blood-vessels rather than with the blood circulating in them.

Whether epistaxis is or is not hereditary is not satisfactorily demonstrated, because, although, amongst those who suffer from it, some may report that it has been known in other members of their family, by far the greater number of sufferers are not acquainted with any

history of the kind, nor do any, according to my own observation, name hereditary predisposition in a marked manner, as they do in respect to other diseases in which heredity plays a distinct part. In almost every instance epistaxis is apt to recur in those who have suffered from it, originally, as a spontaneous symptom; that is to say, as a symptom occurring without a sufficient mechanical cause, such as a blow or a wound. Why it recurs is not always obvious, but two conditions are favourable to it; one, which may be called predisposing, consisting of a dyspepsia often slight in character and usually connected with excessive mental or physical exertion; the other, which may be called exciting, a mechanical excitation, as from blowing the nose sharply, or from a strain in some remote part, like strain during action of the bowels when there is constipation; a rather common exciting cause. The predisposing dyspepsia is not stomachic, but—the expression is quite correct—is intestinal in character, accompanied by constipation and flatulency, with eructations which nearly always are present before and during the epistaxis.

It has become a popular notion that epistaxis is not an unmixed evil. It is said to give relief in certain instances where there have been symptoms of headache, oppression, and lowness of spirits. I am sceptical on this idea, and look upon it merely as a remnant of the old fallacy about the relief obtained from spring and fall blood-letting. No case, at all events, where the epistaxis has been severe has ever come under my observation without leaving behind it weakness, anxiety, and general *malaise*; I am quite sure that whenever epistaxis produces systemic symptoms, symptoms that may be felt by the patient, the effects are so depressing, and demand so long a time for perfect convalescence,

that no reasonable person who has once suffered from epistaxis would wish to have it again.

The colour of the blood in epistaxis is generally of bright arterial red. I fail to remember any variation from this rule. It would look, therefore, at first sight, as if the blood were always derived from an arterial source. It must, however, be remembered that the blood, as it exudes, is widely exposed to the air, by which it is arterialized, and this may account for the phenomenon of colour. The blood is really, as in other hæmorrhages from mucous surfaces, capillary blood, which easily becomes arterial in the presence of oxygen.

In profuse epistaxis, when the blood passes back into the pharynx, it is very common for the patient to swallow a portion of the blood thus escaping. This is sometimes a cause of trouble. The blood swallowed may excite vomiting, by which the measures adopted to stop the hæmorrhage are very much interfered with. Rather acute dyspeptic symptoms sometimes follow when there has been no vomiting, and when the blood received by the stomach is not digested, but, undergoing coagulation, passes through the canal, with the fæces, in the semi-solid form. In a case recently under my observation, in which considerable exhaustion of the body followed what was believed to be a small loss of blood, the exhaustion was easily accounted for by the quantity of blood which passed into and through the alimentary canal.

As people generally pay little attention to epistaxis until the loss of blood becomes very profuse, it is the rule not to call in medical or surgical aid until all "ordinary means" have been brought fruitlessly into service. These "ordinary means" are varied and peculiar. They are chiefly—(a) Cold water applied in douche to the nose; (b) The application of something cold, like the key

of a door, to the nape of the neck; (c) The holding up of the arms of the patient above the head; (d) The placing of the body in a squat position, with the knees drawn up to the body, and the arms clasped over the knees; (e) The effect of air in blast from bellows into the nostril; (f) The introduction into the nostril of something that will absorb the blood or act as a plug, such substances as cotton wool, the fur from a beaver hat, spider's web, and even dust from the road. It is very difficult to decide as to the merits or values of these lines of treatment, since not one of them is trusted alone, and since the bleeding stops, in most instances, spontaneously, so soon as the action of the heart is sufficiently subdued to allow a clot to form and seal up the bleeding vessels. The surgeon may, therefore, discard these empirical plans for his own rational methods, the production of a firm coagulum, and the maintenance of a gentle but decisive pressure. These two objects are met by a good styptic plug, and no plug answers so well as one made of cotton, charged with perchloride of iron. If the cotton be sufficiently moistened to admit of being nicely modelled, it may be made firm and shapeable enough to be passed, like a bougie, along the nasal canal quite into the pharynx, and, formed like a wedge, can easily be made to fill firmly the whole of the canal, as completely as if it were drawn up through the pharynx from behind; and it may, if necessary, be pulled down into the throat through the pharynx, so as to make the wedge the firmer and more secure. After the cavity is thoroughly plugged on the bleeding side, or if necessary on both sides, a gentle pressure may be applied to the nostril with the fingers, until the plug, soon charged with coagulated blood, is firmly set; and this effected, absolute rest for ten or twelve hours is sufficient to effect a cure; but it is

wise not to remove the plug until it can easily be blown out of the nostril by the patient.

I notice, in the *New York Medical Record* for April 19th, 1890, page 444, that Dr. Friedenberg suggests another very simple and effective mode of plugging, which he thinks is as good as Bellocq's canula. He takes a piece of rubber drainage-tubing of small calibre, but of sufficient resiliency, and about ten inches in length. One end of this he introduces into the nasal cavity, and pushes it along the floor of the inferior meatus, through the clots, until it reaches the pharynx. Catching the end in the pharynx with the forceps, he draws it out by the mouth, attaches to it a small compact wad of elastic lamb's wool, rolled in iodoform gauze, and drawing upon the nasal end, slips the wad into the post-pharyngeal space, and stretches the tube until the cessation of all trickling of blood down the post-pharyngeal wall shows that the post-nasal aperture is occluded. Then, still keeping the tubing taut, he packs the anterior nasal recesses with long strips of iodoform gauze all round the tubing, ties a knot in the rubber close to its exit at the nostril, and through it passes a cross piece of tubing of larger calibre, just long enough to fit easily into the nostril. Finally, he releases the end of the rubber, and so leaves in the bleeding cavity a firm plug, which can be easily removed by drawing the tube a little way out and dividing it behind the knot.

It appears to me that this is a most simple and excellent mode of plugging, and that any good styptic like tannin or iron perchloride can be well applied by it. I should prefer it to Bellocq's canula, which I have made use of with less success than I anticipated.

In addition to the local treatment named above, it is always good practice to administer a saline purgative,

of which none can be better than the late Dr. Druitt's
 "Haustus magnesiæ sulphatis acidus:"—

“℞ Magnesiæ Sulphatis ʒj-iv.
 Syrup Aurantii f ʒij.
 Acid. Sulphurici dil. ℥x-xx.
 Aquæ f ʒjss.

Misce. Fiat. haustus.”

This draught may be administered with advantage in a wineglassful of water, twice a day, for a day or two after the hæmorrhage has ceased.

BLOOD-LETTING FOR REDUCTION OF FEVER HEAT.

FIFTY years ago it was the common practice to abstract blood whenever there were symptoms of acute inflammatory fever. In these days it does not enter our minds to do anything of the kind, except in cases of a very special character. In one such case I once drew blood freely under the following circumstances. I was called to visit a lady, who, with symptoms of peritonitis, exhibited a temperature of 109° Fahr. Antipyretic remedies had been freely administered for several hours without any benefit, and at such an elevation of animal heat death was imminent. In the emergency I thought it right to revert to the old practice, and to give the sufferer what might be called a last chance of recovery. Although I had very often drawn blood, I had never before done so with the thermometer as a guide and a corrective, this instrument as an aid to diagnosis having come into practice since the lancet went out. It was, therefore, a new experience to discover what would be the immediate effect on the temperature of abstraction of blood. The blood was drawn from a vein in the arm,

in the usual way, and was received into a vessel holding a clinical thermometer, which, when the bulb of it came into contact with the blood, rose rapidly to 108° Fahr. The abstraction of blood was continued until signs of exhaustion were just felt, and nearly twenty-two ounces were removed. For the time the urgent symptoms were greatly relieved, and the temperature of the body was brought down to 101° Fahr.—eight degrees. The improvement continued for about four hours, when, notwithstanding the administration of ice-water freely, and quinine in large doses, the elevation of temperature recommenced, and continued until it again attained 109° Fahr., shortly after which death took place, with the usual symptoms incident to increment of animal heat.

The clinical lesson derivable from these observations is that while removal of blood has the effect of reducing the febrile heat, it does not control the process of zymosis on which the febrile condition itself depends. It does not, therefore, as a practice, stand favourably above other less extreme measures, such as the application of continued cold to the abdomen or over the region of the heart. Indeed, I have seen such excellent results from such applications of cold, it would never occur to me now to return to venesection for reducing fever heat, however high it might be, or however urgent the condition of the patient.

VOLUNTARY BORBORYGMUS.

NOISY borborygmus, without the control of the patient, is common with some dyspeptics, and I have observed that the symptom runs in families—that is to say, is of hereditary character. But a patient lately consulted me who had the

power of producing loud borborygmus at will. He was a man twenty-nine years old, tall and spare, of bilious temperament, much confined in-doors, a smoker and a tea-drinker, not a total abstainer from alcohol, but moderate in that respect. He suffered from giddiness after smoking, and his general complaint was of nervous debility. In inducing the borborygmus he had either to stand up or sit, in which positions he could make the loudest noises of the kind I have ever heard ; but when he was lying down, either on the back, the side, or the abdomen, he failed altogether to create the disturbance. When standing, the loudness of the abdominal sound and the gurgling which he could induce was so great that it could be heard plainly at a distance of fifty feet. It was produced by a series of regular movements of the abdominal muscles, by which they were contracted and the diaphragm raised. The act was attended by no pain, nor even uneasiness, and he stated that it came at first quite spontaneously, but that having found what the muscular act was, he had reproduced it and kept it up at will, "as if he had learned how to do it." On examining with the stethoscope over the abdomen whilst the noise was being induced, the central spot from which the noise proceeded was found to be in the left hypochondrium, at the point where the colon is just commencing to descend. The noise occurred both in the acts of muscular contraction and of relaxation, but was much loudest during the relaxation, and was divided into two parts by these acts. In the first the sound was dry, harsh, and low ; in the second it was loud, soft, and gurgling, as though fluid had been drawn into the upper part of the colon, and was returning back with a rush into the lower. There was no indication of any tumour in the abdominal cavity, nor, indeed, of any abdominal or thoracic disease. The voluntary muscles

were powerful; but, as is common in these nervous cases, the limbs were tremulous under excitement.

Cases of this kind are the nearest of any to hysteria in the male subject, and are sometimes attended by indication of the globus hystericus, a symptom present in the patient now under consideration. Such patients learn, by a kind of abnormal instinct, how to call into play symptoms of a peculiar order, like this induced borborygmus. The mode in which the phenomenon was brought about seemed to be by muscular pressure on the descending colon at a time when it was distended with gas. Under this pressure, exerted over the distended descending colon, the gas was forced up into the transverse and the ascending portions, and then, when the pressure was taken off, there was a rapid return of the gas into the descending bowel, together with fluid brought over by the current of gas, and giving origin to the loud and gurgling noise. The descent of the diaphragm, and the filling of the lungs with air above, would help in the expulsive movement, but the act was obviously not dependent on respiratory movement, because it could be performed when the breath was held with the thorax at rest.

*WILLIAM CULLEN, M.D., F.R.S., AND THE
GROWTH OF PHYSICAL MEDICINE.*

IN disposing oneself to the task of writing, in a condensed form, the life and labours of a distinguished man, the effort is easy or severe according to the nature of the man rather than the extent of his work. When the work is of one kind, showing the nature of a worker intent on one object, the biographer is relieved of much anxious thought. He can follow his text. When the work is broad and diffuse, not to say versatile, showing a worker of great comprehensiveness, the biographer is soon overwhelmed, and, if he be not careful, lost in the effort that lies before him. Details crowd on details, diversities on diversities, comparisons on comparisons, contrasts on contrasts, until to the bewildered mind, concentration seems impossible.

I have felt this difficulty of concentration most forcibly in studying the life of Cullen. The nature and work of Cullen, in every sense comprehensive, seem to admit of no compression. All his biographers have evidently been impressed with this feeling, and in their attempts to present their man in perfect view have become diffuse unto weakness. The most laborious of all the lives of Cullen is the worst offender in this respect, from which fact I gather the consolation that inasmuch as the



Asclepiad. No. 26, Vol. VII.

London, April 1890.

William Lullen

*Autotype from an engraving by
J. G. Green, 1772, after a painting by
W. Boucher. 1892.*

present essay must needs be short, in order that it may run in harmony with its predecessors, it may perchance be a sharper picture than many that have preceded it.

ON THE ROAD TO FAME.

If the reader can imagine himself entering an apothecary's open shop in Henrietta Street, Covent Garden, London, in the year 1730, a shop belonging to a Mr. Mather, apothecary, he will find behind the counter, vigilant in business, a Scotch youth of twenty years. Our youth, plain William Cullen, who, young as he is, having been born on April 15th, 1710, has by this time passed through a good experience. He has been at the University of Glasgow as a gown student; he has been apprenticed to a member of the Faculty of Physicians and Surgeons, of Glasgow, a rather learned man, Mr. Paisley; he has left Glasgow for London, after having acted as surgeon to a merchantman during a voyage to the West Indies; and now, on his way back to his native Hamilton, in Scotland, he has tarried awhile in London, gleaning a little practical knowledge of *Materia Medica* under Mr. Mather, the apothecary of Henrietta Street. He works hard in the shop, and, according to his employer, he works hard also at his studies when the business of the day is over. In conversation William is a careful youth. Let the subject of conversation with him be one on which he has little or no knowledge, he will listen and take no part; but speak to him at some subsequent period on the same subject, and beware, or he will confound you with his information. He has studied up the matter, until his tenacious memory has enabled him to retain a little library of knowledge, so well applied that he appears to be master of the position entirely. Is it a place that is under discussion; you would suppose he must have been to it:

is it a substance ; he must have handled it : is it a book ; he must have read it : and this faculty he is destined to carry and exemplify throughout the whole of his life.

William Cullen had, at this time, himself alone to depend upon. He had long been fatherless. His father, an attorney and factor to the Duke of Hamilton, married Miss Robertson, of Whistleberry, and very quickly had a family of nine children—seven sons, William being the second, and two daughters. After the birth of the youngest of this family, the father died, and his mother married another attorney of Hamilton, named Nasmyth, by whom she had two children. The eldest son of the Cullen family went to the law. William, after going to the grammar-school at Hamilton, where he was under a teacher of great local reputation, Mr. Findlater, took, as we have seen, to physic as his profession, began his career as a surgeon of a merchant vessel, and afterwards entered as an assistant to a London apothecary. The London engagement lasted until the beginning of the year 1732, when, his elder brother having died, he returned to Hamilton, to assume his place as the head of the Cullen family, and to hold in his medical charge the son of Captain Cleland, under whom he had made his voyage to the West India Islands.

On returning to Scotland, Cullen did not go direct to Hamilton to establish himself, but to Auchinlee, in the parish of Shotts, near to Hamilton, near also to a place where his father had held a small patrimonial estate. He remained in this place for two years, when, having come in for a little legacy, he removed to Rothbury, in Northumberland, to reside with a Dissenting minister there, and to study philosophy. He enjoyed two years in this manner, and then went to Edinburgh as a medical student in the University, took part in forming the society

of students which afterwards became the famous Medical Society, and having passed through his studies, returned in 1736 to Hamilton, for regular professional work as a surgeon, and as attendant-in-ordinary to the Duke and Duchess of Hamilton, an appointment which brought him practice amongst the richest families the country round. Here he received as a pupil William Hunter, destined afterwards to be the famous Dr. William Hunter, and here, in November, 1741, he married Anna Johnstone, daughter of the minister of Kilbarchan, in the county of Renfrew, a charming wife, with whom he held affectionate companionship for forty-six years, and by whom he had seven sons and four daughters. The illness of the Duke of Hamilton caused him to move from place to place, for nearly two years, in company with that nobleman, after whose death, in 1744, he removed to Glasgow, in the University of which city he had graduated M.D. in 1740.

So soon as he was settled in Glasgow, Cullen began to deliver a course of lectures on Medicine, and in the year 1745 he commenced to lecture in the University, with the design of following the example that had been set by Monro Primus, in Edinburgh, of forming a proper medical school. His first course of lectures, within the University walls, was on the "Theory and Practice of Physic," delivered with the consent of Dr. Johnstone, the nominal Professor of that department, who held the chair, but gave no instruction. In 1747 he added to the lectures on Medicine a course on Botany and Materia Medica, in conjunction with Mr. John Carrick, followed by a course on Chemistry, which being adapted to general as well as medical students, was soon a popular course. In the courses of Medicine and of Chemistry he announced many original views, and from the first theorized without stint on points of the most abstruse character. From the pro-

spectus or outline of these medical lectures he afterwards framed the plan of the famous *First Lines of the Theory and the Practice of Physic*, a book which held its place in the libraries of the medical world for over a century. Carrick, in failing health when he joined Cullen in work, was before long removed from him by death, and so all the courses of lectures fell on Cullen alone. He conducted Medicine and Chemistry in the winter, and Botany and Materia Medica in the summer sessions.

Although thus actively engaged in University teaching, and although the University authorities furnished him with a laboratory, Cullen was not actually a Professor, in the strict sense of the word. He really acted for Professor Johnstone, with an understanding that Johnstone should resign in his favour at a favourable and not long deferred opportunity. Some political influence seems to have been required, the appointment being from the Crown, before the Professorship of Physic could be declared. This was brought about by the Duke of Argyll, to whom Cullen was introduced in 1749. Even then two years elapsed before his appointment was formally made, on the 2nd of January, 1751. In Glasgow, life seems to have been irksome to Cullen. His practice was small and widely scattered, his income poor, his labours overwhelming, and the sphere of his usefulness limited. "I am quite tired," he wrote to William Hunter, "of my present life," and he had thoughts of transferring to Edinburgh, to succeed there Dr. Plummer, the Professor of Chemistry. In that project he was supported by Henry Home, better known as Lord Kames. The matter hung fire until 1755, when the illness of Dr. Plummer made it necessary to provide a new Professor. For the chair there were three candidates—Dr. Francis Home, Dr. Joseph Black, and Dr. Cullen. For Cullen, Black had the profoundest regard;

and between them there was no rivalry of a personal kind; still their election was a contest, which, through the active interposition of the Duke of Argyll, ended in the success of Cullen, who, on November 19th, 1755; was elected, by the Town Council, joint Professor of Chemistry with Professor Plummer, during the life of the latter, and with succession to the full Professorship should Dr. Plummer resign or be removed by death. The death of Dr. Plummer was not long delayed; it happened in the following July, and thereupon Cullen reigned in his stead.

It was not altogether a bed of roses he found in Edinburgh. The medical professors were not friendly to a chemical colleague who also practised physic. Added to this, he suffered from a long and tedious illness, from "a broken shin" in the first session of his work, which tried him severely. In 1757 he began to deliver clinical lectures on Medicine, and soon obtained a reputation in that department, the simplicity of his teaching constituting the great charm. The lectures on Chemistry and on Clinical Medicine went on until 1760-61, when the death of Professor Alston, to whom reference was more than once made in the life of Monro Primus, left vacant the chair of *Materia Medica*. At the request of the students, Cullen took up the course, commencing his lectures on *Materia Medica* in January, 1761, and continuing to deliver them until 1766. In the interval much strife arose on the appointment of a new Professor in the chair of Practice of Medicine, on the resignation of Dr. Rutherford, who for some reason did not like Cullen. Cullen, as a candidate for the chair, was backed up strongly by Lord Kames, by William Hunter, and by other distinguished persons; but in the Town Council Dr. Gregory was the favourite, and he carried the day. Later on, by the death of Dr. Whytt, a man of the finest qualities, the

chair of the Theory or Institutes of Medicine was open to competition, and, by a general desire, Cullen allowed himself to compete. He was nominated, and elected to the office, his admission to the Professorship bearing date November 1st, 1766. This new appointment left free the chair of Chemistry, which was filled, according to Cullen's own best wishes, by the illustrious Joseph Black.

The arrangement for Cullen to teach the Theory and Gregory the Practice of Medicine did not please the students of the University. Between one and two hundred of them petitioned that the position of the two Professors might be reversed. After a brisk contention on the point, a compromise was effected: Cullen delivered a course of lectures on Practice of Medicine in 1768, and, until Gregory's death, in 1773, he and Gregory gave alternate courses on Theory and Practice. Gregory died on February 10th, 1773, and Cullen succeeded to the chair.

A little time before he assumed the chair of Practice of Medicine, namely, in 1763, the portrait of Cullen which is published in the present ASCLEPIAD was painted by Cochrane, at the request of many of the pupils of this famous master of physic, now in the fifty-ninth year of his age, and the reigning light of physic in Northern, perhaps in all Britain. For pupils he had men whose names became afterwards the most famous throughout the world, and having now won his way also to practice, he began to receive patients from all parts.

In 1773-4 Cullen presided over the Royal College of Physicians of Edinburgh, and during his presidency published a memorial on the custom of conferring degrees in medicine on persons who had not received a proper medical education, and who might, indeed, receive the degree, *in absentia*, on payment of a fee. Special attention was called to this practice by the notorious case of a man

named Leeds, who, from being a brush-maker, suddenly vaulted into medicine by purchasing a degree from the University of Edinburgh, and advancing to London in 1766, had influence enough to get appointed as physician to the London Hospital. Leeds was a Quaker, and was esteemed amongst "Friends," but he came, nevertheless, under the criticism of another Quaker, the famous Dr. Fothergill, who gave one of his colleagues a warning respecting Leeds: "Take care he does no more mischief." The warning alarmed the Governors of the Hospital, who met soon afterwards, and passed a resolution, to some extent in force up to the present hour, that no one should hold the office of hospital physician who had not passed the College of Physicians of London. To meet this rule, Leeds presented himself for examination before the College, was rejected, and then made an appeal to the Society of Friends to get redress from Dr. Fothergill. The Friends took part with Leeds, and by arbitration awarded him the sum of fifteen hundred pounds, to be paid by Fothergill, as compensation. Fothergill refused, and in his turn referred the matter to a court of law, which through Lord Mansfield declared in his favour. The College of Physicians next took up the question, when their President, Sir William Browne, made a statement, that Edinburgh did not possess a University, and had no power of conferring degrees; and this led to the memorial by Cullen above named. Cullen admitted that all the Scottish Universities had committed the mistake of conferring degrees without examination or proper proof of education; and he held the practice to be specially hard on the Royal College of Physicians of Edinburgh, because that College was obliged, by its charter, to grant licenses, without examination, to any person who had obtained a degree from any of the Universities of Scotland. As

a practical remedy, he suggested that the Universities should "give no degrees but such as are honorary and given without fees, to persons of rank and distinction, or to persons who had given some public specimen of their literature and medical knowledge." The memorial called forth a long and witty letter from the illustrious Adam Smith, who, while not defending the practice of selling degrees, thought the sale did no great mischief, and rather laughed off the whole subject. Cullen was not convinced by Smith's philosophy; on the contrary, he did his best to save Edinburgh the shame of the scandal, and in so far as she was concerned he succeeded. By the course he took he caused her degrees in medicine to be honoured wherever they were recognised.

IN FULNESS OF FAME.

We have now followed Cullen up to the fulness of his fame. He is a figure in history. With strong tenacity of purpose he holds on in his brilliant career, fighting many battles, including a fierce one with the erratic John Brown, not making much pelf, and always determined to follow what, according to his lights, was the right way. Two hundred consultation-letters came to him every year, something in that day thought to be astounding. Honours flow in upon him. The Royal Society of Medicine of Paris elects him an honorary Associate; the Royal Society of London elects him a Fellow (1777). He takes part in turning the Philosophical Society into the Royal Society of Edinburgh, and in 1778 he purchases Ormiston Hall, in the parish of Kirknewton, about eight miles west of Edinburgh. Ormiston Hill is said not to have been a picturesque spot, but he took a fancy to it, and here he spent many of his days. All through his life, in Edinburgh, he had

resided in the same house, in the Mint, not an aristocratic part of the town, but professional and comfortable; and, now, entering into partial retirement at Ormiston Hill, he devoted his attention greatly to farming, but, it is said, with so little of profit to himself that when he died he did not leave sufficient means for the support of his daughters.

Of his sons, Robert, the eldest, born in 1742, became an advocate, and attained the fine position of a Lord of Session. Another, Henry, took to medicine, and showed some talent for his work, but died in 1790, too early to distinguish himself. Archibald, also brought up to medicine, went over to the law, and became a Chancery barrister in England. The other sons seem to have followed commercial pursuits, and to have wandered far away from the paternal home.

In 1786, on August 7th, Mrs. Cullen died; at Ormiston Hill, a loss irreparable to her devoted husband, and one from which he never recovered.

Such, in the briefest language, culled from various available sources, including, of course, Thomson's, or rather the Thomsons'—Dr. John Thomson and his son William—life of Cullen, concluded by Dr. David Craigie, is the picture of the present Master of Physic, up to the time when life was growing weary to him, and the shadows began to lengthen. A beautiful life, near to its end, to which we will not say farewell until, for a short interval, we look at the work of the life. We may then return to the scene of the grand ultimatum.

THE WORK OF CULLEN.

William Cullen comes before us as a phenomenon in medicine. I do not think it would be honest, in the very presence of his greatness, to say that he was an

immortal discoverer. He was original from the beginning to the end of the chapter; he moved medicine from its centre to its circumference, and yet the most careful study of his labours fails to detect now one poor unit of actual discovery with which his name can be connected. In some things too, as in the management of the insane, he was so far behind his time as to favour the treatment of restraint, and even of chastisement. Where, then, did his strength lie? To begin with, Cullen was a man of unfathomable industry. The day of his life was a whole day of work. Work with him was a pleasure, and I warrant that his hours flew like the shuttle of a weaver's beam. To his industry he added the gift of memory. He grasped and held, and whatever his eye swept over, that was his own, never to be lost, never to be given up. Undoubtedly he neglected hundreds of things which he meant to utilize, but which, like a miser, he was obliged to keep, hoping the day would come when he could take them in hand and work them out. He had no selfishness; mammon was never his purchase of the heaven of wealth and repose. Neither was he envious nor jealous; he had not sufficient veneration for the works of any one to feel the pang of envy, uncharitableness, or jealousy. On the contrary, his inbred good-nature incited him to speak fairly of all men, including even his wildest opponent the renowned John Brown himself, amongst the rest. He was fond of teaching, and in the art and act of teaching was more of a student than the students he taught. His heart, too, was in the right place, and he was so intent in work he spent very little time in speculating on what people at large either thought or said of him. For what to men of the Cullen type is this life? "It is even as a vapour which quickly passeth away, and we are gone."

The faculties of the man were well balanced, and so strong that they would have been powerful in any age. It was fortunate for him that they came into operation just at a time when they could be applied with advantage to their owner and to the world. Several new sciences or departments of knowledge were springing up out of veritable darkness, for the advancement of medicine; and what is more remarkable, they were all coming up at once; all new, and yet all correlative; all independent in themselves, and still a phalanx in regard to the place they were to take in the art of healing. Cullen, utterly devoid of the sense and craft of specialism, was made to receive all these developments, to seize their meanings, to try their bearings, and to introduce them, in order, into the medical world.

CULLEN AS CHEMIST.

In the light above stated Cullen was a chemist. Chemistry, when he rose, was emerging from its second stage of development. It had passed through its first days of childhood, of learning to babble and to speak, in terms more or less articulate. It had been a young man rejoicing in its strength, and, full of fond hopes and expectations, had leapt at conclusions which it could not sustain; but now it had reached manhood, and was beginning to rely on experiment pure and simple. It was learning, step by step, and was writing down its primitive lessons in a new method, letter by letter. Cullen came on in this stage, and seeing the right way, pursued it, not as a great unraveller of details, like Boerhaave, but as a pioneer who would cast an eye on promised lands, and report to others the splendid forecasts that lay there. He was methodical in this work. He began to use diagrams in illustrating chemical

problems to his classes, and succeeded in making decompositions and double decompositions clear. He detected the natural lines of research in chemistry, taught that the lines to be followed were those of Nature herself as chemist, and seems to have felt a lasting regret that his other occupations, apart from the chemical, prevented him making demonstrative experiment the basis of his professorial instruction. He gained also a good idea of physical science in some of its branches, especially in relation to cold and heat, and their generation by mixtures. He forecast an agricultural chemistry, and clearly predicted that the produce of the soil would and must, in days to come, be so under the dominion of intelligent man that the earth should be what man would choose to make it, a sterile plain or a fruitful field.

In the Royal College of Physicians of Edinburgh the manuscript lectures of Cullen on the chemical history of vegetables still remain, to testify to his prescience as an organic chemist. They can mean little now as chemical studies of a practical kind, but as history they are as valuable as in the day when they were composed and delivered. Lastly, in the chemical department he turned his mind to bleaching; and might, if he had proceeded with the task, have forestalled the French school of chemists itself in one of the most useful of the industrial arts. In short, as the eminent Professor Robison so admirably has said, "he succeeded in taking chemistry out of the hands of the artists, the metallurgists, and the pharmacutists, and exhibited it as a liberal science, the study of a gentleman."

CULLEN AS PHYSIOLOGIST.

In Cullen's day the science of physiology was beginning to take its proper place in the field of medicine. Like

chemistry, physiology had passed through its preliminary stages of childhood and development towards maturity. It was more advanced than chemistry, for Harvey and the other anatomists had given it a helping hand. Mayow had brought his remarkable genius to bear upon it; Boerhaave had started many new problems of a physiological character, opening up organic analysis. Stahl had speculated ingeniously; Van Helmont had suggested the idea of vital decompositions; Van Leeuwenhœck had brought the microscope into service, and had made his singular discoveries on the construction of the blood and the tissues; Hales had thrown light on vegetable statics; the Monros, *Primus* and *Secundus*, had touched on many physiological problems; and not to name many more, grand Thomas Willis had unveiled the nervous system with a perspicuity which calls for special study before it can be approached properly. Cullen, consequently, was not a founder of physiology; but he was a condenser and interpreter of the works of other men and of nature, and he took occasion to differ freely from his contemporaries, as well as from his predecessors, when he felt he had reason for the difference.

The physiology of Cullen is, to our minds, of this day, singular and yet familiar. It reads like something that has been forgotten. The solid matter of a living body is composed, so he inclines to think, of "a simple solid." Nothing, he said, could be deduced from the chemical analysis of the solids, unless the chemist should be able to recompose the solids from the principles or elements that had been obtained during their decomposition. He held, briefly, that the solids were composed of water and a connecting substance, in its nature a concreting substance, which could, by its concretion, make the water solid enough to be retained as part of itself—a crude view, yet near

the view about colloidal matter which Graham has revealed in our time. He considered that the brain is the first organ formed in the bodies of animals, and consequently is the principal organ, the first to be considered, and the one vital of vital organs. He differed from some of his predecessors in his ideas about the ultimate structure of the solid matter. It had been assumed that the ultimate tissues were cellular in character, an assumption based, no doubt, on the work which had been already accomplished by the microscope. But inasmuch as active structures appeared to be fibrous, and as the cellular character of fibre had not been revealed, he was inclined to the view that solid structure should be considered as altogether fibrous in its nature, although he admitted the existence of a cellular substance on the surface of the fibres. He speculated on the subject of nutrition, and considered that in the earlier stages of development the growth of an animal was similar in kind to the growth of a vegetable, but that, later on, growth in length proceeds by an extension of the arteries, and in width by widening under the lateral pressure of the blood in the arteries. He further thought that the same extensions give rise to secretion, and to the transudation of fluids under pressure into the surrounding cellular tissue. By this extension of the arterial system, he argued, the several parts of the body are gradually evolved—some sooner than others, some later—as, by the constitution of the original stamina or after occurrences, they are severally put into such conditions as render them exposed to the impetus of the blood, and fitted to receive a greater quantity of it. “But as the parts first evolved will, by these causes, increase the most in the density of their solid parts, they will therefore more and more resist their former growth, and by the same resistance will return the blood with

more force and in greater quantity into the parts then not so evolved. Hence the whole system will be at length evolved, and every part of the solids will, in respect of density and resistance, be in balance with every other part, and with the forces to which they are severally exposed." In this view he had arrived at something very near to the true explanation of growth by anastomosis.

On the nervous system Cullen expressed opinions which were considerably different from those which he found advocated by his predecessors and contemporaries, who maintained that the brain is a secreting organ, secreting a fluid necessary for the functions of the nervous system, which fluid, being alternately exhausted and recruited, gives occasion to the alternate states of sleeping and waking. He objected to this view, at least in part. He considered that the whole of the living fibres in the different parts of the body are a continuation of the nerves, and that the nourishment of the soft and homogeneous solids everywhere is conveyed by the nerves. He surmised that the grey or cortical matter of the brain is secretory; that it absorbs from the blood the subtle nutritive part, and transmits its secretion to the white or medullary fibres, and that through those fibres the subtle and nourishing fluid is conveyed over the whole body. By this mode of reasoning Cullen set up a theory quite opposite to that of the Harveian school. The blood, according to his theory, plays a secondary part, and contributes little, if anything, to the nutrition of the tissues. Withdraw the blood, and there will be cessation of vital action, truly, but only because the brain has been deprived of its crude supply, and because the withdrawal leads to collapse of the blood-vessels, an event which must prove immediately fatal.

CULLEN AS PATHOLOGIST.

In the domain of pathology Cullen is supposed by many to have founded a new school. He did not found a new school, but that he propagated one is, I think, a fair and just inference. It seems to me that the foundation of what some have called the Cullenian pathology, especially in relation to fevers and the febrile states, was laid by Hoffman, and that Cullen merely modified and elaborated Hoffman. With the revival of letters the old Galenic obscurities on the phenomena of the febrile state began to disappear, and to be replaced by a rude chemical hypothesis, which attributed fever to a species of fermentation, and made it what would in this day be denominated, in a general, loose, and it might almost be said ignorant phraseology, a "blood disease." Thomas Willis, with his acute reasoning powers, was inclined to the hypothesis of fermentation, or, as we should say, zymosis, as the cause of fever. The mechanical school, which for a period eclipsed the chemical, assigned a mechanical cause. Sylvius de Boe was here the great leading authority. The increased action of the heart and velocity of the blood through the vessels were considered by his school to be the cause of fever, and the quick pulse the sign of the change that was in progress. But why the increased velocity? That was left obscure. Boerhaave evidently detected the difficulty of this question, and in order to meet it ingeniously suggested that in fever there is formed in the blood an acrid substance, "a lentor," which thickens the blood, and gives rise to those obstructions which spring from viscosity. Then followed Hoffman, who seeing, perhaps, that the theory of acidity of blood was deficient, in steps of primary causation, advanced the speculation that fever consists in a modified motion of the muscular fibres, a change induced by nervous influence,

leading to spasm in the minute ultimate arterial vessels. The theory of spasm thus advanced suited the mind of Cullen; it tallied and chimed in with his views of nervous function, and it accorded with his observation of the phenomena of a febrile paroxysm. The cold, the hot, and the sweating stages of fever run a natural course, one after the other; the stages stand in the series of cause and effect, the one to the other. In the cold stage there is universal arterial spasm, the spasm being a kind of *vis medicatrix*; the spasm is a cause of irritation to the heart and arteries, which after a time, like every other overaction, lapses into relaxation, succeeded in turn by profuse sweating and other signs of excessive elimination. The theory was remarkable, but like its predecessors, it failed to go back to primary causes. Why should the minute arterial vessels pass into spasm? It is best to give the Cullenian reply, in the words of its author: "Upon the whole, our doctrine of fever is explicitly this: The remote causes of fever are certain sedative powers applied to the nervous system, which, diminishing the energy of the brain, thereby produce a debility in the whole of the functions, and particularly in the action of the extreme vessels. Such, however, is, at the same time, the nature of the animal economy that this debility proves an indirect stimulus to the sanguiferous system, whence, by the intervention of the cold stage and spasm connected with it, the action of the heart and larger arteries is increased, and continues so until it has had the effect of restoring the energy of the brain, of extending this energy to the extreme vessels, of restoring their action, and thereby effectually overcoming the spasm affecting them, upon the removing of which the excretion of sweat, and other marks of the relaxation of excretories, take place."

The old theory—one of the oldest, he declares, in physic

—that fevers are caused by the entrance of morbid matter into the blood, and that the fever is an attempt to remove away such *materia morbi* from the system, he scouts altogether. “Fevers,” he says, “are produced by cold, fear, and other causes, with all the essential circumstances belonging to the disease, and terminating by sweat without any evidence or suspicion of morbid matter.” The language here is explicit enough, and might be taken to imply that fever is always the result of a nervous impression, which would be very near to spontaneous origin. He qualifies a belief of that kind, however, by recognizing as remote causes of fever some matters floating in the atmosphere and acting upon men, to which matters he gives the name of miasms or contagions. Of miasms he knew one only, that of marsh miasm, the cause of intermittents. Of contagions, he says, there are many, and he thought that for each specific contagious fever there is a specific contagion, but that there is one principal, perhaps one common, source of such contagions. His experience of gaol and other fevers, arising from bad sanitation, impressed him strongly with the idea that the human body, itself left under unfavourable conditions, could generate out of its own secretions poisonous substances, which might give rise to specific contagious disease. He was of opinion that the poisons of contagion act through a limited sphere, extending but a very short distance from the centre from which they emanate, and that their emanation from the excretions of the sick may be more dangerous than when derived from the body itself.

He was exceedingly earnest in his description of the effects of cold. Conversant with the different and, as they appear, contradictory effects of cold,—now a stimulant, then a sedative; now an invigorator, then a devitalizer,—he tried to account, as we still do, for the varying action of

cold by attributing the differences of the phenomena to the differences of degree acting upon the arterial contraction.

Cullen was not great as a morbid anatomist. No doubt he dissected, but in this respect he was far inferior to Morgagni. As a pathologist, however, especially in a theoretical point of view, he showed an ingenuity of thought equal to genius of moderate quality. His pathology was based on his physiology, and so far was good in principle. His pathology of fevers was that the primary phenomena of fevers are dependent on spasm of the smallest arteries. and on the after changes which take place upon relaxation of the spasm. Every stage of fever he maps out by these modifications of function; and the method is so attractive, and, on the premises, so logical, it is neither surprising to read of the success with which it was received nor to recall the length of time it held its ground.

Inflammation he considered as differing from fever in that it is local in its nature, and in that it has its own characteristics. The presence of the buffy coat of the blood, or "the separation of gluten," as he calls it, is, he held, generally pathognomonic of the inflammatory state; but the phenomena of inflammation are all consistent with an increased impetus of blood in the vessels of the part affected; yet, inasmuch as the heart itself need not be influenced at the time nor increased in impetus, therefore the spasm causing the resistance in the inflamed structure is local. The nature of inflammation is defined as a spasm of the extreme arteries, supporting an extreme action in the course of them, in all cases not derived from direct stimuli applied to the part. According to this view, inflammation is a local fever, but one which may become general by extension of the original cause of excitation. If the inflammation is removed whilst the affected structures retain their continuity the termination is by

resolution; which resolution may be, (*a*) from reduced impetus; (*b*) from increased exhalation in the part; and (*c*) from increased impetus of the blood altogether, causing evacuation from the body sufficient to lower the phlogistic condition of the whole system. When the inflammation terminates by suppuration, the gluten of the blood (fibrine) is changed into pus, and abscess is the result; but in the abscess all that has been exuded is not retained; there has been an absorption of the thin or watery fluids into the blood, so that the condensed pure pus alone remains.

Cullen was fully acquainted with the effects of absorption of decomposed exudative matter from inflamed surfaces, and was also conversant with the dangers arising from extravasation into the structure of cellular organs. Thus in inflammation of the lungs the effused matter, by the mechanical impediment it throws in the way of the circulation, may occasion actual suffocation, a mode of death common to inflammations affecting the lungs and the pleural membrane. He was aware of the origin of plastic exudations on serous surfaces after inflammatory attacks, and of the accumulations of serous fluid which sometimes accompany the transudations of plastic material.

In summary, Cullen reduced the remote causes of inflammation into four heads: (1) External stimulation, like the cautery; (2) External violence; (3) Extraneous substances lodged in a part; (4) Cold sufficient to excite over-action, but not sufficient to destroy vitality. It is remarkable that he missed in this study the influence of internal stimulation in causing arterial spasm, but he did so, I believe, absolutely.

CULLEN AS NOSOLOGIST.

There is no work by which Cullen is so well known as by his nosology. This book first saw the light in the year

1769, and passed through many editions, even during his lifetime, the edition of 1785 being the last he himself revised. I remember well that at the time when I became a student, his nosology was still in full force in the schools, and it is still the foundation of nosology, although very much changed. He tells us that he devoted himself to its construction with fear, and only under the impression that the task was a duty. He was not the first, as is generally taught, to invent a nosological chart, but he was the first to construct one that should be at once comprehensive and practical. Some great men, including Sir John Pringle, thought the task a work of supererogation, on the ground that the phenomena of disease are too variable to admit of strict classification; but Cullen, nevertheless, went on and succeeded. He had before him, for models to work upon: firstly, the nosology of F. Boissier de Sauvages, of Montpellier, who led the way on a suggested idea or plan of our own Sydenham; secondly, that of Linnæus, of Upsal; thirdly, that of Vogel, of Gottingen; and fourthly, that of Sagar, of Inglaw, in Moravia.

The works of these authorities Cullen took as his foundations. He commenced by delivering a short course of lectures to advanced students, and from those lectures he constructed the outlines of the nosology, elaborating, step by step, until the whole was completed. He reduced the number of classes of disease named by his predecessors; brought the classes into orders, and the orders into genera, his object being to form a classification of the diseases which affect mankind, according to their natural history, by grouping together those symptoms which, without hesitation, may be admitted to be so frequently combined as to indicate a derangement of the body, springing from a common cause, and coming, properly,

under one name as a single disease. He was himself fully aware that this nosology would change as the study of disease advanced, but felt that he had sufficient to enable him to show a good beginning, and to leave some groups in such natural order as to remain, so long as diseases remain, on a natural and unchangeable foundation. There can be no doubt that he succeeded in this admirable attempt beyond expectation. Any one who will cast an eye over the nosology will be astounded to re-discover, if his recollection of the great undertaking has become impaired, how much of it still holds good.

Cullen was, *par excellence*, a universalist in medicine and in science, a circumstance which prevented him from giving local affections a first place in his tables; yet in spite of what some have considered a defect in this respect, he drew too near to commanding principles to be very far from the series of natural characters of all diseases, local as well as general; for even nowadays, let us split up diseases into as many local divisions as we may, the conscience, together with the best knowledge of the practitioner, is alive to the truth that all local forms of disease are really subordinate to the general. Our tendency in these days is to classify disease according to producing cause. In Cullen's time the attempt was to classify according to symptoms. Perchance Cullen, after all, was nearest to the right method.

CULLEN AS THERAPEUTIST AND PRACTITIONER.

As a therapist Cullen was simple in all that he did. He was given to administer one thing at a time, and to test the effect of one remedy, carefully, before he proceeded to another. He discarded many remedies which had become literally worn out, but which clung to men with superstitious tenacity. He wrote a book on *Materia*

Medica, which was a text-book of its day, although now long obsolete and a curiosity rather than an authority.

In practice Cullen was favourable to depletion, and in the life of John Brown (ASCLEPIAD, No. 16, Vol. IV., published in 1887) some account will be found of the great controversy on repletion *versus* depletion, which waged between the two physicians and their followers. Cullen was a blood-letter, but he was not so irrationally. He was led, on logical grounds, to this practice on the view that spasm of the arteries is the direct cause of the febrile and inflammatory states, and that the most immediate method for reducing spasm is to draw blood. Bearing on this point I have before me a letter from my distinguished friend Dr. George Balfour, of Edinburgh, so entirely expressing my own views that I quote it in its entirety. Speaking of the blood-letting practice in pneumonia, Dr. Balfour says that Cullen "was justified in his views by the prevailing science of the day, which taught that inflammation was due to spasm of the arterioles, and this, if not checked early, ended, as pathology was supposed to teach, in rupture of the bloodvessels, escape of blood into the lung tissue, and death possibly preceded by suppuration; red or grey hepatization being always present. But Cullen believed that the most efficient cure for spasm is blood-letting, and he strongly recommended this in pneumonia, not because his patients were more asthenic than they now are, but because he was no fool, and, with his light, regarded blood-letting as the only effectual remedy. In his *First Lines of the Practice of Physic* (p. 332, 7th ed.), he tells us that in his day there were many ready to faint on a small bleeding, yet who might with care have as much blood taken as was necessary, since they bore bleedings much better than the first. In a copy of

his "*First Lines*" in our Library (Library of the Royal College of Physicians of Edinburgh) there is a MS. note in his own handwriting, pointing out that it was in his day no uncommon thing for patients to faint on the abstraction of only four ounces of blood; yet that there was no difficulty, even in these cases, in pushing blood-letting as far as was thought necessary."

This is a just estimate of the action of Cullen in the matter of depletion; but we have to turn to the "*First Lines*" themselves in order to see their author, in his ripeness, as a practitioner. The first complete edition of this work was published in the year 1784, and the preface to it, bearing date 1783, is of itself a study, revealing, in brief and yet forcible tone, the man as physiologist, chemist, practitioner, and scholar. Stahl, as he thought, favoured only the treatment of disease by expectation. Hoffman he extols as the founder of a new era in medical treatment, though he traces the elements of that foundation to Thomas Willis in his *Pathologia Cerebri et Nervorum*. Boerhaave, whom he first followed, he now ventures to dissent from in some particulars, and whilst he maintains that the only useful work in the practice of physic is making a collection of all the facts relating to the art, and of all that experience has taught us with respect to the cure of diseases, he doubts if the same can be accomplished without aiming at some system of principles, by a proper induction and generalization of facts. He praises the study of morbid anatomy, as leading towards a knowledge of the proximate causes of disease. He endeavours to collect facts relating to diseases of the body as fully as the nature of the work will permit; but he has not been satisfied with giving the facts without endeavouring to apply them to the investigation of proximate causes, and upon these

to establish a more scientific and decided method of cure. Further, in order to obviate any dangerous fallacy in proposing a method of cure, he has always been anxious to suggest that which, to the best of his judgment, "appeared to be the method approved of by experience, as much as it was the consequence of system." Such, very shortly stated, is the scheme of the practice of Cullen, as set forth in the "*First Lines*," long the textbook of the schools of physic all the world over.

POST-MERIDIAN.

The reputation of Cullen, rather slow to rise, seemed never to decline after it had once attained its height. He was consulted on all medical subjects at home and from abroad. His admiring disciples were everywhere spreading his name and his fame. The official personages asked him for information, and were satisfied. Lord Cathcart extracted from him a description of the best mode of striving to recover persons who are drowned; and the reply, although it does not convey anything that needs repeating, was accepted, at the moment, with the utmost gratitude. One event only could now prove a check on the career of William Cullen, and that was the event which shall certainly overtake the strongest of us—old age and decline.

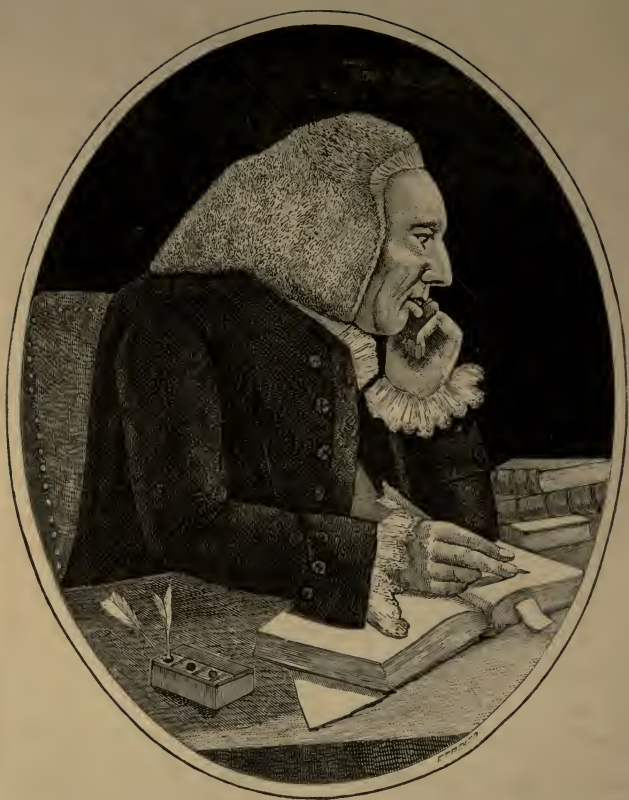
The death of Mrs. Cullen, in 1786, told upon him powerfully; and it is doubtful whether Ormiston Hill was the precise place for his repose. On a gate at the northern outlet of the "Dingle," leading to the mansion, he had engraved the words from Horace, "*Procul Negotiis*," and on the front door of the mansion he had engraved, from the same poet, the words "*Est Ulubris*." The meaning of these sentences has been differently understood. It seems simple enough that the first should

mean retirement, aloof from business ; but the second, which refers to the solitary place where Augustus is said to have been educated, and which Juvenal describes as "*Vacuis Ulubræ*," is not so comprehensible ; unless it means, as Thomson supposes, that a contented mind may be happy even in a desolate spot. However, there it was, and Dr. Balfour, to whom I am indebted for the autograph of Cullen, bearing date 1783, on the frontispiece of this ASCLEPIAD, informs me that over the doorway of the house, now the servants' offices of a finer mansion, there it remains : "*Est Ulubris*."

In his prime Cullen was rather a handsome man in feature, but he had a stoop which probably took from his height. Benjamin Rush, one of his pupils, states that his face was long, his eyes blue but soft, his nose large and inclined to point downwards, his under lip protruding over the upper. "Over his whole face," says Rush, "was diffused an air of mildness and thought which was strongly characteristic of the constant temper and operations of his mind."

The portrait of Cullen painted by Cochrane, and engraved by Valentine Green in 1772, which forms the frontispiece of this number, bears out this description, and in the rooms of the Royal Medical Society of Edinburgh another portrait of him, painted by D. Martin and engraved by Bengo, and, in later days, by Holl, conveys much the same impression. Martin's portrait shows Cullen in his sixty-seventh year, and would be a fine work of art but for a hideous grimacing bust of Hippocrates which the artist has thrust in, in order, it may be supposed, to make the Father of Medicine the admiring father of the great Professor.

Dr. Balfour informs me there is a plaque by Tassi, engraved by Ridley, old and ugly, with hanging under-



*Portrait of Cullen, by Kay, bearing date 1787, from copy
in the possession of Henry Veasey, Esq., F.R.C.S., of
Aspley Guise, Beds.—B. W. R.*

lip. I have not seen this, but I present the reader with another very interesting portrait which, by lucky chance, I lighted on unexpectedly. Cycling, one fine summer afternoon, through the pretty village of Aspley Guise, near to Woburn, in Bedfordshire, I stayed, to receive a most kindly welcome from my old friend Mr. H. Veasey, and in looking over the pictures in his study my eye fell on the portrait of Cullen, copied, by permission of my friend, for this memoir, and engraved, with the skill for which he is so appreciated, by Mr. Charles Ferrier. The plate bears date 1787, and was done by Kay, the well-known engraver and etcher, who drew the sketch of Lord Monboddo, with a tail. Kay, an uneducated man endowed with great artistic talent, was noted for giving the characteristic features of those who came under his pencil, but he could never manage a figure. He caught his portraits "flying;" that is to say, at a glance. Whitefield, Wesley, and other celebrities, were thus caught by him, and now his originals are much prized. In this portrait of Cullen he has caught the great physician during his last days, with shoulders high, face lengthened, and lower lip partly paralysed and fallen; a failing man in his seventy-eighth year.

For two years after the date above named Cullen continued to carry on the duties of his chair. He resigned it in 1789, being nominally succeeded by Dr. James Gregory. The resignation was an occasion for a series of the most interesting events of respect and recognition. The Lord Provost, the Town Councillors, the Magistrates, and the principal Citizens of Edinburgh, presented him with a piece of plate, bearing an appropriate inscription. The students of the University raised a subscription for a marble bust, now in the

University Library, by the sculptor Gowens. Addresses were presented to him from the *Senatus Academicus*, and from many of the learned Societies on whose roll his honoured name appeared.

William Cullen continued gradually to decline, without evidence of any special form of disease, until February the 5th, 1790, when he died in his house in the Mint, Edinburgh, aged fourscore years within one month. He was buried, by his own request, without public ceremony, at Kirknewton, the parish in which Ormiston Hill is situated, on Wednesday the 10th of February, 1790, nearly in the centre of the burying-ground. No written memorial marked the spot, and it was only by a slab set up to his son, Robert Cullen, the Lord of Session, who died in 1810, that the tomb of the far more illustrious man was recognisable. In the year 1864 the Council of the Royal College of Physicians of Edinburgh placed over the dilapidated tomb a new tomb, with a medallion above the entrance bearing an appropriate inscription.

From Dr. Alexander Keiller, of Edinburgh,—to whom I am indebted for the photograph of the tomb of Cullen, from which the engraving opposite has been executed by Mr. Ferrier,—I learn that in the Library of the Royal College of Physicians of Edinburgh there are still retained many volumes of the manuscripts of Cullen, and at least one copy of his lectures, with numerous books purchased at the sale of his library in 1791.

And now another task is done. To reduce such a life of variety and work as the life of Cullen to a few pages; to be resolved to leave nothing of major moment out; and, to be equally resolved to put nothing of minor moment in, has, indeed, been a laborious task. But,

throughout, the labour has been lightened by the sweetness of it, by the communion of it, and by the richness of the classical ground that has had to be traversed. Sometimes, in writing these characters anew, one learns to love them, sometimes to pity them, sometimes to study them without any intensity of feeling whatever. Each one conjures up some new sentiment, which the reason balances. Towards William Cullen sentiment and reason alike suggest profound respect; and at the gate of his final resting-place I offer this tribute to his memory.



BURIAL-PLACE OF CULLEN AT KIRKNEWTON.

REPORTS, PHYSIOLOGICAL AND THERAPEUTICAL, MADE TO THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FROM 1863 TO 1872.*

I.—ON THE PHYSIOLOGICAL PROPERTIES OF
NITRITE OF AMYL.

REPORT II.*—READ AT SUB-SECTION D., ZOOLOGY AND BOTANY
INCLUDING PHYSIOLOGY, ON FRIDAY, SEPTEMBER 16th,
1864, AT BATH.

[EDWARD SMITH, M.D., F.R.S., IN THE CHAIR.]

Continued from page 87 of ASCLEPIAD, No. 25, 1st Quarter, 1890.

Further Observations on Warm-blooded Animals.

N warm- as in cold-blooded animals the nitrite produces cataleptic conditions, but not so prolonged. Administered gradually by inhalation to a strong rabbit until complete prostration was induced, I laid the animal on a table and found that the respirations were reduced to one per minute. The limbs were flaccid and motionless; and when they were moved and were laid in any given position, there they remained. The pupils were widely dilated, and the red portions of the body, as the mucous membranes of the mouth and eyelids, were absolutely white; the action of the heart could not be felt, nor was it certain that the motion could be heard with the stetho-

* In this Report repetitions in the original are omitted, and a few sentences are abbreviated.

scope. Certainly the two sounds were lost. In this condition, breathing softly but sharply once in sixty or eighty seconds, the animal continued for two hours; then the breathing gradually rose. In three hours and a quarter the action of the heart could be felt by the hand; in three hours there was movement of the limbs, and in five hours the animal had recovered.

In an experiment performed by Dr. Gibb and myself, a cat was rapidly struck down by being placed in a thousand cubic inch jar through which the vapour from one fluid-drachm of the nitrite had been diffused from a surface of bibulous paper. Death took place in two minutes. The animal was removed and was watched with great care, but the breathing had ceased. The pupils were dilated to their fullest extent. After a time we laid open the chest. On exposure to the air, the heart began contracting most vigorously, and soon the muscles of respiration also commenced spontaneously to contract, moving the ribs, and disturbing the abdominal viscera. The diaphragm contracted very steadily, and a muscle of the thigh, on being laid bare, did the same. These contractions actually continued spontaneously from twenty-four minutes past twelve until forty-eight minutes past one in the day—a phenomenon which has, I believe, never before been observed after death of warm-blooded animals in any of the muscles except the heart.

The heart itself, in this case, continued contracting on the right side when all the other muscles were at rest. To observe the local action of the nitrite on the heart, we gradually instilled three minims of it on the right ventricle. The muscular structure soon became of a dirty white, but the contractions continued. At seven in the evening the right auricle, with a segment of the ventricle, was still contracting five times in the minute; at ten

o'clock it was contracting in the same way, although the lower limbs of the animal were rigid from *rigor mortis*; at twelve (midnight) it was contracting at the rate of two per minute; at one it was reported by Dr. Henry as contracting strongly from one to two beats per minute; at five a.m. I found it contracting three times in a minute and a half, and at eight it made a contraction on being touched with a needle. For many hours before this all the other muscles of the body were rigid. Thus there was witnessed the strange phenomenon of muscular contractility of the heart for nearly eighteen hours after death and *rigor mortis*.

If instead of administering the nitrite of amyl by the mouth, or by the lungs, it be injected under the skin with a hollow needle, it induces, though more slowly, the same symptoms.

Local Effect of the Nitrite.

I have made some careful observations on the effect of the nitrite upon the capillary vessels of the web of the frog's foot. The results are very uniform and decisive. A few seconds after the web is treated with the nitrite the capillary vessels are seen to dilate to more than twice their natural calibre, and the rate of motion of blood is immensely quickened. After an interval of fifty or sixty seconds the vessels become tortuous as from irregular contraction of their walls; then there follows a decided narrowing of the vessel at its minutest parts, which continues until at last the vessel becomes indistinct, and all motion of blood is lost, except a faint oscillation in vessels which are running transversely into a main current. These experiments were confirmed by observations made by my friends Dr. Henry and Mr. Yeats.

Effects on the Blood.

The blood of animals destroyed by the nitrite may always be smelt as charged with the substance. On a large animal that had been killed by the injection of forty minims I drew off an ounce of blood from the right side of the heart into a flask, and on inhaling from the flask, absorbed sufficient of the nitrite vapour to induce the specific signs of its action. The fluid, however, in no way interferes with coagulation, but as I have said before, it arrests oxidation and decomposition. On the corpuscles it exerts a powerful osmotic action. It has no effect on them in the way of dissolution, nor does it, when added to them, destroy their form or modify the central depression, but it reduces them in size, leaving them well defined, and capable of running together in the ordinary way.

Summary.

From these experiments we may learn the following facts on the physiological action of the nitrite of amyl.

1. It is absorbed by the bodies of animals however introduced into the organism—by the skin, by the stomach, by the lungs, by the cellular tissue.

2. After its absorption its effects are seen immediately on the heart and circulation; there is in the first instance violent action of the heart and dilatation of the capillaries, followed by diminished but not extinguished power of the heart, and contraction of the extreme vessels. As an excitant of vascular action, the nitrite of amyl may be considered the most powerful agent yet discovered.

3. On animals, such as frogs, from whose bodies it can be removed spontaneously, and whose circulatory and respiratory systems are simple, the nitrite suspends animation, but when the animals are placed under favourable conditions for the process of recovery, they may recover after

considerable periods of time. There is no other known substance that suspends animation in these animals for so long a period. On warm-blooded animals, whose circulatory and respiratory systems are more complicated, the nitrite cannot actually stop the movements of respiration and circulation without destroying life. But even in these animals it can, without destroying life, reduce the forces of respiration and circulation so extremely that a condition precisely analogous to what is known as trance or catalepsy in the human subject, can be brought on and sustained for many hours.

4. The nitrite of amyl is not an anæsthetic. By it consciousness is never destroyed, until a condition approaching to death be produced.

5. The effects of the nitrite on the organism are directed to the motive powers, which it excites and then subdues.

6. The *modus operandi* of the nitrite appears to be by arresting the process of oxidation in the tissues.

7. Physically the nitrite holds a place between the volatile bodies, such as chloroform, and the solid bodies, such as opium and woorali. Hence the effects are less evanescent than those of the very volatile substances, and less certainly destructive than the solid substances. In this lies the secret of its prolonged action.

PATHOLOGICAL EFFECTS OF THE NITRITE OF AMYL.

In cases where the nitrite of amyl is carried to its extremest effects the appearances of the internal organs present some modifications. The appearances are not the same in every instance, but vary according to the mode in which the substance is administered. If it be administered very quickly, the lungs are found blanched and empty of blood; the right side of the heart is engorged with blood and the left side is empty; the brain

is free of congestion. If the substance be administered slowly the lungs are congested, the brain is congested, and blood is found both on the right and left sides of the heart. The organs of the body are also of a dirty reddish-brown colour, and the blood is similarly discoloured, no distinction in colour existing between the arterial and venous bloods. Notwithstanding the violent action of the heart, I have never seen rupture of any vessel nor extravasation of blood. The inner lining of the blood-vessels is unchanged, and the valvular mechanism of the heart maintains its integrity. It is to be remembered that these observations have all been made on healthy animals.

COMPARISON OF EFFECTS OF THE NITRITE WITH OTHER
AMYL-COMPOUNDS AND OTHER CHEMICAL BODIES.

I have compared the action of nitrite of amyl with certain other of the amyl-compounds, but I have not had sufficient time to complete this line of research, as each new compound opens up for itself a new field of observation rich in variety. As yet I have only tried the comparison with amylene and acetate of amyl. Amylene differs from the nitrite in that it acts as an anæsthetic; but it resembles the nitrite in exciting the circulation in a minor degree and in causing redness of the skin. The symptoms produced by amylene are, however, very transitory as compared with those following the use of nitrite; there is this in common, that neither of them entirely destroys consciousness, but amylene destroys sensibility, which the nitrite does not. The acetate of amyl, in comparison with the nitrite, seems to me to produce a more marked local and a less general effect. It causes on inhalation, not only redness of the face, but swelling and soreness of the mucous surfaces, without any violent excitation of the heart. From chloroform the nitrite differs in that it does

not produce anæsthesia; and the same remark applies to ether, the monochloruretted chloride of ethyle, nitrous oxide gas, Dutch liquid, turpentine, and ammonia, although it resembles all these in that it excites the circulation in the early stages of its action. The only substance which approaches the nitrite in action is woorali, a vegetable compound which is much the same in its elementary composition. Woorali produces less preliminary excitement of the circulation, it paralyzes more determinately all the muscles except the heart, and being a solid substance, possessing no means of escaping from the body except in solution, it is more slowly eliminated.

CAUSE OF THE RAPIDITY OF THE CIRCULATION.

Why the nitrite of amyl should produce such suddenly increased action of the heart is a point of great physiological interest. I thought at first that this must be an effect primarily manifested on the blood, then on the heart, and, through the increased impetus of the heart, on the capillary circulation. It was, however, soon apparent that the injection of the capillary system was too quickly developed to be a sequence of mere over-action of the pulsatory power of the central organ of the circulation, and the experiments on the web of the frog's foot settled the question, I think absolutely. It is possible that the action of the nitrite is exerted immediately upon the extreme filaments of the vaso-motor nerves, and that the heart beats quickly because the resistance to its force is taken off by the dilatation of the minute vessels which it supplies with blood. At the same time the vascular currents of the heart itself are quickened, and its movement is intensified proportionately.

On the facts so far presented in this report, two questions call for a moment's consideration. The first

is:—Whether we ought dogmatically to deny the possibility of the human body being in such a condition that it may for some hours, or even some days, assume the appearance of temporary death? We are conversant of rare cases of disease, called cases of trance or catalepsy, in which life, seeming for an interval suspended, is restored; we have heard of other cases in which it is said that certain natives of India who are called Fakirs, produce, by some secret art, an imitation of death so determinate that the most intelligent are deceived. I cannot but feel, after what I have seen in the experiments on which the present inquiry is based, that the explanation of the cataleptic state admits of a better solution than ever before it did, and that the validity of the Fakir experiment is rendered, at the least, probable. I doubt not that in catalepsy there is formed in the body itself a chemical substance which, without actually stopping the motions of the heart and of respiration, suspends them so nearly that passive life only is carried on, and that this condition is continued until such time as the substance is removed from the circulation. I conceive it is also quite reasonable to presume that the Fakir has at command some substance, derived from the vegetable world, which, more effective than the agent that has been before us this day, possesses the power, when introduced into the body, of suspending the common signs of animation for a certain number of hours, and that “in this borrowed likeness of shrunk death” the facts of the phenomena are presented.

The second question is:—Whether, from what we have learned in this inquiry, any knowledge may be gathered relative to the application of the nitrite of amyl as a remedy in disease? I have been too closely and intently occupied in the task of obtaining elementary facts, to

devote time to the practical elucidation of this important point. But, subject to further and better experience, I should infer that in cases where in a healthy organism sudden death is apprehended from failure of the heart, as, for example, in syncope from severe pain, fright, or inhalation of chloroform, the cautious administration of the nitrite by inhalation might call into action the failing organ, and give it time to recover from the shock to which it has been subjected. Again, I believe that in tetanus the nitrite might be employed with advantage. Paralyzing the extreme filaments of nerves, and reducing the muscular power of all the voluntary muscles in the same manner as does woorali, the nitrite possesses advantages over woorali which the man of science will at once recognise. It is more easily administered; it does not necessarily destroy the power of the muscles of respiration, and it is much more easily removed from the organism by excretion. It might therefore be employed in tetanus, to suspend the violent spasm, and give the system time to throw off the primary evil.

As regards the mode of administration of the nitrite. It may be given by direct inhalation; by inhalation with chloroform and ether; or by the mouth as an alcoholic tincture in doses of three, five, or ten minims, or even in larger doses, according to the effects produced. It would be safest to use it in small doses by frequent repetition.

*FROM REPORT READ IN SECTION OF BIOLOGY DEPARTMENT,
OF ANATOMY AND PHYSIOLOGY, ON FRIDAY, AUGUST 24TH,
1866, AT NOTTINGHAM.*

[PROFESSOR HUMPHREY IN THE CHAIR.]

I have experimented to ascertain whether nitrite of amyl, which, as we have seen, exerts so decided an effect in

quickening the action of the heart when it is inhaled by the living animal, might be turned to account as a means for stimulating the heart into action in instances where that organ has suddenly ceased to beat, as in cases of fainting, in drowning, in sunstroke and lightning-stroke, in death by chloroform, and by suffocation.

For these experiments I invented a new instrument for transfusion, which works so simply and effectually that I may be excused, perhaps, if I diverge for a moment to describe it: the practical physiologist will find it of great value in many inquiries. This instrument consists of a glass cylinder, with a flexible tube running from its lower part or chamber, for insertion, by means of a quill or hollow probe, into the vein to be injected; the upper part of the cylinder is provided with a stopper, through which a tube passes, connected with a small pair of hand bellows. Within the cylinder is a small hollow ball, or safety-valve regulator, which floats, if there be any fluid in the cylinder, until the fluid allows it to descend to a constricted neck at the lower part of the cylinder, when all further passage of fluid is prevented. The flow of fluid along the escape-tube can be governed by a clip.

In using this instrument, we first place the fluid to be injected in the cylinder, and let a little run through the escape-tube to displace all the air; next we close the escape-tube by means of the clip; then, having opened the vein or artery, we insert and fix the quill or hollow probe at the end of the escape-tube, and when all is ready for the fluid to float we remove the clip and raise the cylinder two or three feet above the body. The ordinary fluid pressure will now usually suffice to carry the fluid into the body equably and gently; but if there be any obstruction the merest pressure of the floating hand bellows will remove it. As the fluid descends the ball goes down

with it to within three inches of the bottom of the cylinder, where it is opposed by the constricted neck, so that no air can possibly get into the blood-current.

The experiments made with nitrite of amyl for resuscitation have, so far, all been negative, but they have nevertheless taught an interesting and useful physiological lesson. They illustrate that when in the living body the nitrite of amyl excites the heart to vigorous action, producing suffusion of the skin and the other extreme symptoms of excitation, the effect is conducted solely through the nervous system, and that the action of the nitrite, telling, at the moment of inhalation, upon the extreme filaments of the olfactory nerves, as well as on the pneumo-gastric tracts, communicates a peculiar and rapid motion, which traverses nerve, and without any direct action on the blood, reaches the heart, giving to it impulse and vehemency of action.

The experience of every-day life tells us that the heart may be thrown into similar activity by impressions or influences communicated from the external world to the senses, and through them to the heart. The influences of sounds, harsh or melodious—of sights, appalling or fascinating, are well known, from the manner in which they come upon us. From their invisibility of action, if I may be allowed such an expression, we are prone to look on these as immaterial agencies: they are not so; thoroughly understood, they are physical shocks like the impress of a liquid or gaseous substance.

Nitrite of amyl is one of those substances which enable us to realize this connection between the really material and the seemingly immaterial influences which surround us. We take an appreciable quantity of it, say half a grain, and inhale it from paper, and at once we feel a quickened action of the circulation so decisively that we trace the

effect immediately to the cause. We could, if we liked, quicken the heart to absolute silence by pushing the cause far enough. Here there is no mistake, no possibility of mistake. But we can modify the experiment, and refine upon it. By admixing the vapour of ammonia with that of amyl, and diffusing the combined vapours through a large space of air confined by walls and closed windows, we can charge a room with a compound which the olfactory sense, as such, does not detect, but which tells with active and peculiar force upon the heart. In this way an invisible and, as it would seem to the unlearned, an immaterial agency acts in obedience to the human will. I could, for example, make every heart in this room rise ten beats, at least, within a minute, without diffusing a detectable odour, as surely as I could vary the motion of a steam-engine by moving the lever.

The day will soon come when we shall know the mode by which these agencies act upon the body through the nervous expanse; we shall follow out the living organism as so much matter movable and transformable or transmissible, built in, and I had almost said upon, a refined medium, itself unchangeable, all-pervading, and establishing a bond of union between our own material parts, ourselves, our planet, our universe. We shall discover how this fluid, itself physical, subjected to various influences, is disturbed, and how it communicates its disturbances to the grosser matters which it permeates; then a vast number of strange and, as they now appear to us, conflicting phenomena will resolve themselves into a single and simple law, and physiology, in its wholeness, the science of the sciences, will be the most useful and the most exact.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience ; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."

"PHYSIOGNOMY: A SYSTEM OF PRACTICAL AND SCIENTIFIC PHYSIOGNOMY; OR, HOW TO READ FACES."

[BY MARY OLMSTED STANTON.]

IN two large and handsome volumes (published by F. A. Davis), Miss Stanton, in the most elaborate and painstaking manner, presents her studies to the world. So imposing a labour, on a subject of a scientific nature, has probably never before been placed in the libraries of science by a lady, and it is a striking evidence of the change which is coming over our learned as well as our social life, to see so remarkable an effort. To say that this authoress outvies authoresses would be an injustice to her and to authoresses in general. She outvies modern authors of both sexes on the subject of physiognomy, and Lavater himself need not have been ashamed of such a record of research and observation. The book, moreover, is profusely illustrated, so that the artist's as well as the reader's eye is educated. With the physiognomy phrenology is blended, and some old-fashioned people will say that Gall,

Spurzheim, and George Coombe figure in thought and teaching, as fully as Lavater or Camper. The system followed, while grounded on phrenology of an advanced kind, is also physiological. Each new part opens or develops something in relation to organic structures and functions of particular parts, in systematic order. Take, for example, the eye as a physiognomical feature. In the first place, the eye is described anatomically; next, some lessons are given as to its function. Then follows its character as a feature, and the readings which may be obtained from it in a physiognomical point of view. The descriptions are illustrated by portraits, showing the different types of human eye; the normal, the abnormal, the secretive, the superstitious, the polygamic, the conjugal, the truthful, the acquisitive, the observing, the untruthful, the politic, the linguistic, the artistic, and so on. The same method is followed in regard to the nose and the other parts of the face.

Like all authors of her school of thought, Miss Stanton is inclined to be absolute in her deductions, which shows her earnestness of belief, but which subjects her to the certain fate of being entirely believed in by the minority, whose sympathies or mode of education are influenced in her direction, and to be utterly disbelieved in by the majority who do not accept her lines of exposition and inference. She will doubtless find consolation in the largeness of the minority who will be loyal to her views, and who are certain to wear her arguments and demonstrations threadbare in their enthusiasm. Unquestionably she has a large audience, and has chosen, or fallen upon, a time when also a favourable audience is re-appearing. The doctrine of localization of faculties and of motor centres has recently been advanced in pure physiological circles, and many persons who heretofore

have listened to the very name of Gall and the phrenologists with bated breath, are now speaking of him and them with a tinge of admiration. We may expect, therefore, in the future, to hear often of Miss Stanton's excellent work as one of increasing influence and popularity; a result which the industry, learning, and skilful construction bestowed upon it richly deserve.

"CHRONIC ALCOHOLISM."

[BY JOSEPH FRANK PAYNE, M.D., F.R.C.P.]

THIS small but important work by Dr. Payne is an address introductory to a discussion on the morbid anatomy and pathology of Chronic Alcoholism, held at the Pathological Society of London on December 4th, 1888, and is reprinted from the *Transactions* of the Society of 1889. In the book there is one sentence which, coming as it does from so good a pathologist as this author, is worth any price. I set the sentence out, therefore, alone in all its powerful simplicity:—

"On the whole, I submit that the general results of the facts adduced is in favour of what I may call *the toxic theory of the action of alcohol on tissues*" (p. 30).

This statement is the practical outcome of the argument that the analogy of the other forms of neuritis produced by various toxic substances, such as lead, arsenic, copper, or by specific diseases, in which the histological characters are the same as those of alcoholic neuritis, though the vascular disturbances are wanting, supplies a strong argument in favour of the toxic action of alcohol. These agents also exert the twofold action which the author has

attributed to alcohol, namely, of producing parenchymatous degeneration and interstitial inflammation.

The observations given above are in a reply made by Dr. Payne to the various criticisms which had been passed on his paper by the Fellows of the Pathological Society. It is necessary for the reader to turn to the paper itself in order to get a full insight into the argument which led up to the reply. In this paper there will be found, in order, an account of the effects of alcohol as seen (a) *in the liver*; (b) *in cirrhosis of the liver*; (c) *on the nervous system*; (d) *on the spinal cord*; (e) *on the peripheral nerves*; (f) *on other organs* besides the liver and the nervous system, namely, the *kidneys*, the *organs of generation*, the *lungs*, and the *skin*. It is the fact that on every one of these parts the author finds a morbid anatomy from alcohol. Speaking of general paralysis, he discovers for that condition three factors: alcohol, functional strain, and, in many cases, congenital incapacity to bear strain, *i.e.*, “a disproportion between functional activity and power of resistance, especially in the higher cortical centres and the tracts connected with them.” He emphasises this suggestion because it appears to confirm the conclusion arrived at on other grounds, “that the effects of alcohol on the nervous system, and even on the brain, are independent of its functional effect on the nerve cells, but are those of a *tissue poison* acting directly on the protoplasm of various parts.”

One peculiar feature is present on every page of this treatise. Not a single name is mentioned of any one of the many writers who have discussed the action of alcohol on the body from the practical side of temperance. Why is this strange omission? Other authors are named. For what reasons are those most interested, those who through storm of unmerited attacks have borne the brunt of

speaking first the truth, the whole truth, and nothing but the truth, altogether omitted? The reasons for the omission are few in number. Is it that these men are all wrong? Impossible, if Dr. Payne be right, for they have said precisely what he says. Is it that Dr. Payne is afraid to recognise a great social and moral as well as scientific movement, which must in course of time become so powerful as to cover with the lasting credit and gratitude of a reformed world all who have led the way to the grand reformation? This can hardly be the case; for if current belief be true, Dr. Payne is one of the very limited few who would like to live after death, even in this world; live as one who has left the world a little better than he found it. Is it want of knowledge? No; for he is one of the best read men in medicine. Is it that the course pursued is wisely and quietly strategic in favour of the great reformation? The chances are that this last is the true reason. It is most probable that, wishing to do his best, and knowing the littleness of mankind, the price of prejudice, and the danger of opposing the rude Cæsarism of the age, he has followed to the letter the maxim of combining the wisdom of the serpent with the gentleness of the dove. Well, be it so. "All's well that ends well," and Dr. Payne both begins and ends well.

"ASPHYXIA."

[BY GEORGE JOHNSON, M.D., F.R.S.]



R. GEORGE JOHNSON is one whom everybody delights to honour. Since the death of Sir Thomas Watson, to whom he most appropriately and tenderly dedicates the book now under notice, there has been no better, no purer, representative

of the medicine of our day, in its highest development. Everything that he writes and says, of a professional kind, is so simply sincere, and so noonday clear, that pleasure and trust must flow from it, even in diversity of view and opinion.

I have felt all this in reading the short treatise (J. & A. Churchill, 1889) in which he once more sets fully forth his ideas on the cause and nature of asphyxia. He contends that when the chest is laid open immediately after death by asphyxia, the right cavities of the heart are distended with blood, while the left contain comparatively little blood; that the final distension of the right cavities, with comparative emptiness of the left, is the result of an impediment to the passage of blood through the lungs; and, that the only probable explanation of the obstructed pulmonary circulation is that which attributes it to extreme contraction of the pulmonary arterioles, consequent on complete deoxygenation of the blood. The phenomena, he thinks, are characterized by two distinct stages. In the first stage there is systemic arterial resistance, with resulting distension of the left side of the heart. In the second stage there is pulmonary arterial resistance, with distension of the right cavities, and comparative emptiness of the left.

On the whole, I should agree with Dr. Johnson in regard to the pathology here described; but, as will be seen from the first article in this number of the *ASCLEPIAD*, I should make a distinction betwixt asphyxia and apnœa, which he does not, but which I hope he will, in his future observations. The work, from the first page to the last, is one commanding the closest attention of the physician as well as the physiologist. It is as practical as it is theoretical, and combines theory and practice with singular felicity.

"A MANUAL OF ANATOMY FOR SENIOR STUDENTS."

[BY EDMUND OWEN, M.B., F.R.C.S.]

IN construction and intention this book (Longmans & Co.) is one of the most original that has appeared for many years past as a work of reference in medicine and surgery, founded on anatomy. It recalls the *Praxis* of Sylvius and the *Anatomy* of John Bell. The anatomy of an organ is first described, and then follows the practice, medical or surgical, that may, in the course of the labours of the practitioner, be connected with the anatomy. To assist in the elucidation of the text there are abundant illustrations, and, as a whole, there is compressed into 511 pages a library of practical information, coming upon us as a foretaste of a return to comprehensible as well as comprehensive learning. That the book will find its way into every medical library goes without saying. It is born and bound to win its position and to thrive on time.

TO CORRESPONDENTS,

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD is exclusively by the Author whose name is on the title-page.

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD should be addressed to the Author at his residence, 25, Manchester Square, London, W.

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, Paternoster Square, London, E.C.

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

May 15th,



1890.

APPENDIX OF ACKNOWLEDGMENTS.

- ALEXANDER.** "Substitutes for the Opium Revenue." By JOSEPH G. ALEXANDER, LL.B. London: King & Son. 1890.
- "**AUSTRALIAN** Health Society's Health Lectures for the People." 2nd Series. Melbourne and Sydney: George Robertson & Co. 1889.
- BANKS, LINNÆUS.** "From the Same Nest." By Mrs. G. LINNÆUS BANKS. London: *Church Monthly Office*. 1889. [One of the charming and natural little stories for which this well-known and excellent author is so much admired. Medical readers, not less than general, will read it with profit, as showing human nature in natural portraiture of scientific as well as artistic worth.]
- BLAKESLEY.** "A Table of Hyperbolic Cosines and Sines." By THOMAS H. BLAKESLEY, M.A. London: Taylor and Francis. 1890.
- BROWNE.** "Treatment of Retention of Urine." By G. BUCKSTONE BROWNE, M.R.C.S. London. 1889. [A good practical essay.]
- BURMAN.** "Suggestions for the More Rational Therapeutical Treatment of Respiratory Affections." By C. CLARK BURMAN, L.R.C.P. Edin. 1889.
- BURRELL.** "Fracture of the Spine." By HERBERT L. BURRELL, M.D. Medical Society, Massachusetts. 1887.
- CAMPBELL.** "Flushing and Morbid Blushing." By HARRY CAMPBELL, M.D. London: H. K. Lewis.
- COHEN.** "A Case of Sarcoma of the Thyroid Gland." "Apparatus for the Therapeutic Use of Compressed and Rarefied Air." By SOLOMON SILAS-COHEN, A.M., M.D. *New York Medical Journal*. 1889.
- COLLINS.** "Specificity and Evolution in Disease." By W. J. COLLINS, M.D. London: Collingridge.
- COLLINS.** "Rationalism and Freethought in Medicine." By W. J. COLLINS. London: Adlard & Son. 1890. [Another of the author's brief and characteristic essays. Very thoughtful, but not equal to the essay on Spinoza in carefulness of exposition and depth of understanding.]
- DAVIS.** "On Anæsthetics in Operations for Adenoid Growths of the Naso-pharynx." By HENRY DAVIS, M.R.C.S. London: Lewis. 1890.
- DOBRÉE.** "A Manual of Home Nursing." By LOUISA EMILY DOBRÉE. London: Swan Sonnenschein & Co. 1889.
- ELLIS.** "The Criminal." By HAVELOCK ELLIS. London: Walter Scott. 1890. [A book which will demand a longer notice, and which should command a large circulation.]
- FARRINGTON.** "The Use of Alcohol in Medicine." By J. M. FARRINGTON, M.D. *New York Medical Journal*. 1889. [In every sense truthful, usefully suggestive, and attractive.]
- FENWICK.** "Electric Illumination of the Bladder and Urethra." By E. HURRY FENWICK, F.R.C.S. Eng. London: J. & A. Churchill. 1889. [In this volume Mr. Fenwick continues to open new and advanced views on one of the most striking developments of physical science applied to surgical art.]
- FOOT.** "Medical Selection for Lives for Assurance." By A. WYNNE FOOT, M.D. Univ. Dublin. Dublin: John Falconer. 1890.
- FORMAN.** "Poetry and Prose of John Keats." Edited by J. BUXTON FORMAN. London: Reeves & Turner. 1890. [John Keats again lives in this most interesting volume.]
- FREUND.** "Ueber die Anwendung des Ichthyols bei Frauenkrankheiten." By DR. H. W. FREUND. Sonderabdruck der Berliner Klinischen Wochenschrift, No. 11. 1890.

- FRICK.** "Ar puro a Domicillo nas Cidades Tropicacs." By JOAO FRICK. Rio de Janeiro: Leuzinger & Filhos. 1889.
- GADDE.** "Beitrag zur äusseren und inneren Anwendung des Ichthyols." Von PROF. MRS. ASN. GADDE. Sonderabdruck aus Therapeutischen Monatshefte. 1890.
- GLOVER.** "The Gaps in Medical Education." By J. GREY GLOVER, M.D. London: Baillière & Co. 1889. [Dr. Glover's "Gaps" have the advantage that they will be well filled up when his advice in regard to them is followed by our responsible hedgers and ditchers.]
- HALE.** "The Water Cycle Technically and Practically Considered." By J. MORTON HALE. London: Hiffe & Co. 1889.
- HARKIN.** "The Vagus Treatment of Cholera." By ALEXANDER HARKIN, M.D., F.R.C.S. London: Henry Renshaw. 1890.
- HILL.** "Chronic Urethritis and its Treatment." By M. BERKELEY HILL. London: H. K. Lewis. 1890.
- HOPKINS.** "The Johns Hopkins' Hospital Reports." PUBLICATION AGENCY OF THE JOHNS HOPKINS UNIVERSITY. Baltimore. January 1890. No. 1, Vol. II. [The work of the great Institution named in the title is epitomised in these reports with rare ability and judgment.]
- JONES.** "Public and International Hygiene." "Diseases of the Nervous System." "On the Use of Antipyretic Remedies in Febrile Diseases." "Investigations into the Nature, Causes, and Prevention of Endemic and Epidemic Diseases." By JOSEPH JONES, M.D. New Orleans: Joseph Jones. 1889.
- JONES.** "Observations on the Cerebral Blood-vessels in Normal and Morbid States." By C. HANDFIELD JONES, M.B., F.R.S. London: Baillière & Co. 1889.
- JONES—EMRYS.** "The Medico-ethical Teachings of Oliver Wendell Holmes." By A. EMRYS-JONES, M.D. 1889.
- KNOTT.** "Tinea Sycosis." By JOHN KNOTT, M.A., M.D. Dub. London: John Bale. 1890.
- LIEBIG AND ROHE.** "The Practical Application of Electricity in Medicine and Surgery." By G. A. LIEBIG, JR., PH.D., and GEORGE H. ROHE, M.D. Philadelphia and London: F. A. Davis. 1890.
- MCCLELLAN.** "Note on Rumbold's Method of Treatment of Catarrhal Inflammations of the Upper Air Passages." By ELY MCCLELLAN, M.D. Chicago. 1889.
- MARIANI.** "Coca and its Therapeutic Application." By ANGELO MARIANI. New York: J. N. Jaros. 1890.
- MASSEY.** "My Lyrical Life." Poems Old and New. 2 Vols. By GERALD MASSEY. London: Kegan Paul & Co. 1889.
- MAXWELL.** "The Vision of the World: a Forecast of the Future." By DAVID MAXWELL, C.E. Hull: A. Brown & Sons. 1890.
- MAY.** "On the Pathology and Treatment of Diphtheria." By EMANUEL MAY, M.D. Hei. London: J. & A. Churchill. 1889.
- MICHAELS.** "De la Restauration de Maxillaires." Par M. J. P. MICHAELS. Chatouraux. 1890.
- NUSSBAUM.** "Ueber den inneren Gehiranch des Ichthyols." Von DR. VON NUSSBAUM. Munchen, Berlin. 1888.
- OSLER.** "Tubercular Peritonitis." By WILLIAM OSLER, M.D. Baltimore. 1890.
- OWEN.** "A Manual of Anatomy for Senior Students." By EDMUND OWEN, M.B., F.R.C.S. London: Longmans & Co. 1890.
- PAYNE.** "A Discussion on Morbid Anatomy and Pathology of Chronic Alcoholism." By JOSEPH FRANK PAYNE, M.D., F.R.C.P. London: Adlard & Son. 1890.
- PÉAN.** "De l'Ablation totale des os de la face." Par M. PÉAN, Chirurgica des Hopitaux de Paris. Chatearoux. 1890.
- PEPPER.** "Hepatic Fever." By WILLIAM PEPPER, M.D., LL.D. Philadelphia. 1890.
- PEPPER.** "The University in Modern Life." Pennsylvania. 1890. "Benjamin Rush." By W. PEPPER, M.D. Chicago: Office of the Association. 1890.

- PHILLIPSON.** "Sewage Disposal from Country Houses." By BURTON R. PHILLIPSON, F.R.G.S. London : R. D. Webb & Son. 1889.
- POPE.** "The Beginnings of Disease." An Address at the West London Medico-Chirurgical Society. By H. CAMPBELL POPE, M.D., F.R.C.S.
- REPORT** of the Homes for Inebriates Association of the Dalrymple Home at Rickmansworth. 1890.
- RITCHIE.** "An Australian Ramble." By J. EWING RITCHIE (Christopher Crayon). London : T. Fisher Unwin. 1890.
- ROGERS.** "Celticism a Myth." By J. CRISTOPHER ROGERS. London : E. W. Allen 1889.
- ROOSE.** "Gout and its Relation to Diseases of the Liver and Kidneys." By ROBSON ROOSE, M.D., LL.D. London : H. K. Lewis. 1889.
- "**RULES** for Management of City Hospital for Infectious Diseases." Newcastle-upon-Tyne : Andrew Dickson. 1890.
- RUSSELL.** "Anniversary Address on the Teaching of Chemistry to Students of Medicine." By W. J. RUSSELL, F.R.S. London : Harrisons. 1890.
- SCHULTZE.** "Athetbewegung und Herzaktion Asphyktischer Früchte." Von B. S. SCHULTZE. Aus dem Centralblatt für Gynäkologie. 1890.
- SCHULTZE.** "Ueber die Wiederbelebung tief Scheintodt Geborener Kinder." By DR. B. S. SCHULTZE. Wien : J. Bergmann & Co. 1885.
- SHARP.** "A Study of Doses." By WILLIAM SHARP, M.D., F.R.S. London : G. Bell & Sons. 1890.
- SMITH.** "Relation of Micro-organisms to Diseases in Man." By W. R. SMITH, M.D., F.R.S. Edin. London. 1889.
- SPICER.** "Nasal Obstruction and Mouth Breathing." "Throat and Nose Affections in Children." By SCANES SPICER, M.D., B.Sc. London : John Bale & Co. 1890.
- STANTON.** "A System of Practical and Scientific Physiognomy." By MARY OLMSTED STANTON. Philadelphia and London : F. A. Davis. 1890.
- STONEY.** "Studies in Ontology." By G. JOHNSTONE STONEY, M.A., F.R.S. London : Harrisons & Sons. 1889.
- "**THEORY** of a Basis of Temperament and of a Probable Cause of Organic Disease." Anon. 1889.
- THIRION.** "Traité de l'Economie Domestique." Paris : Henri Jouve. "The Teaching of French in English Schools." London : Simpkin & Co. "The French Irregular Verbs." By A. THIRION, LL.A. London : Hachette & Co. 1888.
- THOMAS.** "On the Climatic Treatment of Phthisis." "Abstract of a Clinical Lecture on the Localisation of a Cerebral Clot." "On Some Cases of Lead Poisoning from Drinking Water." By W. R. THOMAS, M.D., M.Ch. London : John Bale. 1889.
- THOMPSON.** "An Address to the Australasian Association in Melbourne 1889." By DR. ASHBURTON THOMPSON. Sydney : Cunningham & Co. 1890.
- THOMPSON.** "A Record of the Sanitary State of New South Wales." By J. ASHBURTON THOMPSON, M.D., D.Ph. Sydney : Charles Potter. 1890.
- THOMPSON.** "Transactions of the Royal Academy of Medicine of Ireland." Vol. VII. 1889. Edited by WILLIAM THOMPSON, M.A., F.R.C.S. Dublin : Fannin & Co. 1889.
- "**TRANSACTIONS** of the Hastings and St. Leonards-on-Sea Health Congress." Hastings : F. J. Parsons. 1890.
- TUCKEY.** "Psycho-Therapeutics ; or, Treatment by Hypnotism and Suggestion." By C. LLOYD TUCKEY, M.D. London : Baillière & Co. 1890.
- TURNER.** "Address to the Anthropological Section of the British Association at Newcastle-upon-Tyne 1889." "The Cell Theory : Past and Present." Inaugural Address to the Scottish Microscopical Society, November 1st, 1889. By SIR W. TURNER, KNT., M.B., LL.D., D.C.L., F.R.S.S.E. and L. Edinburgh : Neill & Co. 1890.

TURNER. "The Physician—a Naturalist." A graduation address delivered August 1st, 1888. Address at the opening of the Anatomical Department in the New Medical Buildings of University of Edinburgh. By Sir W. TURNER, KNT., M.B., LL.D., F.R.S.S.L. and E., F.R.C.S. Edinburgh: Oliver & Boyd. 1888.

WRIGHT. "A Brief Memoir of the late J. O. Halliwell-Phillips, LL.D., F.R.S., F.S.A." By GEORGE R. WRIGHT, F.S.A. London: Whitting & Co. 1889.

THE WILLIAM F. JENKS MEMORIAL PRIZE.

The Second Triennial Prize, of Four Hundred and Fifty Dollars, under the Deed of Trust of Mrs. William F. Jenks, will be awarded to the author of the best essay on

"THE SYMPTOMATOLOGY AND TREATMENT OF THE NERVOUS DISORDERS FOLLOWING THE ACUTE INFECTIOUS DISEASES OF INFANCY AND CHILDHOOD."

THE conditions annexed by the founder of this prize are, that the "prize or award must always be for some subject connected with Obstetrics, or the Diseases of Women, or the Diseases of Children;" and that "the Trustees, under this deed for the time being, can, in their discretion, publish the successful essay, or any paper written upon any subject, for which they may offer a reward, provided the income in their hands may, in their judgment, be sufficient for that purpose, and the essay or paper be considered by them worthy of publication. If published, the distribution of said essay shall be entirely under the control of the said Trustees. In case they do not publish the said essay or paper, it shall be the property of the College of Physicians of Philadelphia."

The prize is open for competition to the whole world, but the essay must be the production of a single person.

The essay, which must be written in the English language, or if in a foreign language, accompanied by an English translation, should be sent to the College of Physicians of Philadelphia, Pennsylvania, U.S.A., before January 1st, 1892, addressed to Louis Starr, M.D., Chairman of the William F. Jenks Prize Committee.

Each essay must be distinguished by a motto, and accompanied by a sealed envelope bearing the same motto and containing the name and address of the writer. No envelope will be opened except that which accompanies the successful essay.

The Committee will return the unsuccessful essays if reclaimed by their respective writers, or their agents, within one year.

The Committee reserves the right not to make an award if no essay submitted is considered worthy of the prize.



ARTIFICIAL RESPIRATION: THE THEORY AND THE PRACTICE.

THE essay in last ASCLEPIAD on Asphyxia, Apnoea, and Syncope of the Lesser Circulation, leads, naturally, to the study of artificial respiration, its services, and the best mode of carrying it into practice. The subject is one which, for the last hundred and twenty years, has, from time to time, called forth the study of the experimentalist, and after him the practitioner; but nothing as yet has been sufficiently carried out in a strictly scientific and uniform manner. When the moment arrives—always an urgent moment—for artificial respiration to be demanded, no two practitioners seem ready to follow the same plan, and few have had sufficient experience to decide, absolutely, which is the best plan. In some of the public institutions for the recovery of the drowned one method is followed, in others another; but comparisons have been wanting, hitherto, of sufficient value to lead to any sound and reliable line of practice on which to establish a definite principle of action. It is my wish in this paper to attempt to point out how to arrive at something like uniformity of action both in principle and in procedure.

In order to make a methodical as well as a concise essay on artificial respiration, let us study the subject

under four heads: (*a*) the theory; (*b*) the practice; (*c*) the value; (*d*) the expectation.

THE THEORY OF ARTIFICIAL RESPIRATION.

The theory of artificial respiration is, that by restarting the process of respiration immediately after it has ceased to be carried on by the natural process of breathing, the natural process may be restored, and life—in some cases, at all events—may be saved. Experiment has shown that, for a period of three minutes after air has ceased to be drawn in by inspiration, the vital functions may still be carried on by the reserve air in the lungs. Under practice a longer term of suspended life may be acquired, as in divers, one of whom, to my own knowledge, could remain under water, and therefore without respiring, for five minutes and twenty seconds; but we may accept that in the majority of persons three minutes is the full time of possible suspension of human life, in the absence of respiration.

After the body has ceased to live on the reserved air, various changes rapidly occur, even when the obstruction to respiration is of the simplest kind. Some obstruction to the entrance of air into the lungs, or congestion of blood in the lungs, may be the direct mechanical embarrassments; the blood in the pulmonary vessels may coagulate, and render the most perfect artificial respiration useless; the arterioles of the pulmonary artery may be firmly contracted, so that no blood can be carried to meet the air on the respiratory surface, however skilfully air may be sent to that surface; the right side of the heart may be temporarily overwhelmed with blood, or the left side may be incapable of contraction or dilatation; and, lastly, the muscles of respiration may be incapable of action, either from spasmodic closure or by the opposite state, that

of paralysis. Theory, therefore, strictly interpreted, would teach that artificial respiration can only be expected to succeed in cases where the bronchial tract and vesicular structure of the lung is free; where the blood is fluid; where the heart is in motion; and where a current of blood is still passing over the pulmonic circuit from the right to the left side of the heart.

In carrying out artificial respiration, in any manner whatever, it is necessary, according to theory, to maintain the balance of the circulating and respiring systems. I mean by this the balance of pressure between the blood impelled into the pulmonic circuit by the contraction of the right ventricle of the heart and the pressure of the air that has to pass into the lung to meet the blood. In health this balance is perfect, and the integrity of the vascular and vesicular structures of the lungs depends on the continuance of this perfection. Some recent statements, to the effect that artificial respiration conducted with great pressure of air has been successful, are so utterly inconsistent with all that has been observed, and with the natural method of breathing, that I am forced to let them pass as conveying some great error, or as being performed on too small a scale to be of any service in common practice. When, under suspended respiration, the propelling power of the heart is reduced, as it always is under such circumstances, it is necessary to restore the respiration with the gentlest action possible. It is good, at such moments of action, to recall the simple experiment of bringing an expiring candle-wick into flame by breaths of air. The two processes are identical: the operator is blowing into life a flame of life which is all but exhausted, and which the least rudeness may extinguish for good.

A sound principle to be observed relates to the repose

of the body during the process. The body cannot be resting too quietly whilst the lungs are being filled and emptied of air. The reason of this is that the heart has to be considered as well as the lungs. Any excessive motion interferes with the cardiac contractions, mechanically, and any muscular movement exhausts the circulation just at the moment when the restoration of power calls for the fullest conservation. The respiratory muscles, and especially the diaphragm, demand also every possible influx of force for their work, upon the renewal of which life entirely depends. The position of the body, both for the work of the heart and of the diaphragm, should be exactly in the horizontal line, and nothing should be allowed to take it from that line except the removal of fluid or other substance causing obstruction.

It is important that the air introduced into the lungs should be warmed. If there be no better means at command than an open fire, the advantage of that ought not to be lost. The body should be brought as near as possible to the fire, and the warm air from the fire should be used for the inflation. But the true thing is a warm room, like an engine room or hot-air bath room, in which the air, in every part, is full 90° Fahr.

The quantity of air to be thrown into the chest artificially is a matter of much moment, and one to which I have at various times paid a considerable amount of attention. The vital capacity of a full-grown, well-developed, healthy man shall be, say, 376 cubic inches. He can take this measure of air into his lungs at once if he make a special effort. But even in the case of a man having this capacity, not more than fifteen cubic inches are required at each inspiration for the support of good vital action. I made this important point a matter of direct experiment on myself, and found the measure of

air named fairly sufficient so long as I remained in repose, either in the erect, the sitting, or the recumbent position. In the performance of artificial respiration we may, therefore, accept 15 to 20 cubic inches of air as the minimum to be used at each introduction of a charge of air into the lung. It may be assumed that the introduction of four times this quantity would not be too much; but it may also be assumed that the smaller amount named is, under most circumstances, sufficient.

Another theoretical point relates to the diaphragm. When artificial breathing has been set up, the action of the diaphragm, so important in natural respiration, is lost. This fact cannot be over-estimated, for the diaphragmatic contraction is the chief agent under the natural process. It would seem, therefore, *à priori*, to be good practice to excite the diaphragm. But theory indicates, at this present stage of our progress, that to excite the diaphragm, by stimulation of it, is to interfere unfavourably with the action of the heart. We have, consequently, if we follow theory, to let alone the great inspiratory muscle, and leave it to resume work in the course of its own natural revivification.

It has been suggested, theoretically, that in inflating the lungs artificially it would be advisable to use pure oxygen, or an air containing an excess of oxygen, instead of common air. If the theory were true it would be embarrassing, because during the sudden emergencies in which the artificial method is demanded the means of obtaining pure oxygen would, usually, be difficult. Fortunately, theory in this case is not supported by experiment; for I found by direct comparative experiment that the attraction of blood for the oxygen of the common air was better than for oxygen alone in the artificial process. The presence of the atmospheric nitrogen seemed to have a

good effect by distribution of the oxygen into the vesicular structure.

THE PRACTICE OF ARTIFICIAL RESPIRATION.

The practice of artificial respiration may be carried out in one of the following ways:—(a) by the manual method, or direct method, as some describe it ; (b) by immediate insufflation, or the mouth-to-mouth method ; (c) by intermediate insufflation, or the bellows method ; (d) by insufflation combined with manual expiration ; (e) by electrical excitation, simple or in combination with insufflation. These plans have all their own advantages and disadvantages. In order to estimate their respective merits, we will take them into consideration in the manner in which they are arranged above.

THE MANUAL OR DIRECT METHOD.

This method, the one usually resorted to in cases of drowning and other mechanical forms of death by asphyxia, is brought into operation in many different ways. The simplest is that which was followed by Cullen and his school after the discoveries of Black had put aside mouth-to-mouth insufflation. This manual process consisted simply in laying the patient on the back, making pressure firmly on the chest and abdomen, so as to empty the chest of air, and then setting the compressed parts at liberty, and allowing the chest to refill by its natural resilience and expansion. It is not to be denied that this simple method has many advantages, and in young subjects, in whom the resiliency of the chest walls is good, it is an effective procedure. In stillborn children it is very effective, and it is not likely to pass out of use. It remained the only system of its kind for over fifty years ;

until, in fact, the ingenious Marshall Hall replaced it by the system which bears his name.

The Marshall Hall plan is carried out by laying the body in the horizontal position, and giving it a gentle movement from the side towards the fore part of the chest, and back again, in what may be called cradle fashion, the arm being close down by the side. With the backward movement the body should be pulled a little over towards the back. This mode of artificial respiration is simple, and in most cases ready. It has the effect of partially emptying the chest of its contained air, and of refilling it with outside atmospheric air; and, from its simplicity, it has been much patronized. Its disadvantages are that it is difficult to carry it out on a soft, yielding surface like a bed, and on the narrow operating-table.

Another manual process is that introduced by Dr. Sylvester, and known by his name. It is performed by moving the arms of the patient. The body is laid prone, or with the trunk somewhat elevated, and the arms are raised by the elbows above the head, and are afterwards brought down to the sides, repeating the act several times a minute. This is a very good plan indeed; it causes the chest to expand and draw in a fair volume of air, and it empties the chest of contained air, by easy movements.

A third manual method was brought into practice by Dr. Howard of New York, and bears his name. It is performed by placing the patient on his back, after emptying the chest and throat of any contained water; putting a hard roll of clothing beneath the back; bending the head well backwards so that the throat may be on the stretch to the fullest degree; fixing the hands of the patient over the head by a tie, a handkerchief, or piece of cord; and, finally, kneeling astride the patient's hips, grasping the front part of the chest towards the pit

of the stomach, the thumbs pointing to the chin and the fingers fitting into the grooves between the short ribs, and firmly pressing the sides of the patient together, bringing at the same time the whole weight forward, so as to empty the chest by compression, and leaning back to let it empty itself by the recoil of the ribs, repeating the movements from seven to ten times a minute.

A fourth manual method has recently been introduced by my friend Dr. Andrew H. Smith of New York. In this method the patient, in the recumbent position, is placed, if possible, on a table, at which are *two* operators. One operator, at the head of the table, takes a hand of the patient in each of his, and draws the arms upwards with a slow and steady pull, continuing the traction until the maximum of thoracic distension is obtained, in about three seconds. The traction is then relaxed, upon which the other operator, kneeling or standing by the side of the patient, presses with both hands forcibly upon the chest in a direction backwards and towards the median line, so as to diminish both the depth and the breadth of the lower half of the thorax, which expiring pressure is continued for two seconds. Both sets of actions are repeated twelve times a minute. This method, a combination of Sylvester's and Howard's, is effective without being laborious. Its disadvantage is that it necessitates two operators.

Lastly, under the direct manual processes may be named a plan suggested by myself, which consists in placing the body of the patient in the sitting position, and letting the right hand be firmly grasped by the right hand of one person, the left by the right hand of another person, standing on their respective sides. Each operator now lifts the hand he has in charge upwards in an oblique direction, until the limb is well stretched, and raises it with sufficient force to lift up the body very slightly.

Then, simultaneously, the two let the arms down to the side, flexing the forearm on the upper arm, and bringing the upper arm in close proximity to the side with firm pressure, using the forearm as a lever, and repeating these acts ten to twelve times a minute. By the first of these acts the thorax is made to take in a good volume of air, and by the second it is made to throw out an equally good charge. The advantages of this method are that the labour required of each operator is not severe, and that in the process of raising the body the diaphragm descends, making inspiration very nearly natural. The disadvantages are that two persons are required to keep up the operative procedure, and that a third person should take part in holding up the body in the sitting position.

THE IMMEDIATE INSUFFLATION METHOD.

This method, which predates the time of Black and the discovery that carbonic acid is given off by the breath, was advocated by Dr. Fothergill, who flourished in the last century. By it the operator instilled his own breath into the lungs of the person whom he was trying to restore. He placed his mouth to the mouth or nostrils of the patient, or, as was the more common as well as more desirable practice, he made a funnel of his left hand, placed that by the outer part over the nostrils, and closing the mouth of the patient, and putting his own mouth to the opening of the funnel formed by his thumb and forefinger, he breathed through the funnel backwards and forwards so as to empty and fill the lungs of the patient. The plan thus devised was considered, at first, effective; but, after the discovery that in expiration the poisonous gas, called originally fixed air—carbonic acid—is given off by the lungs, it was argued that to pour the exhaled breath of one human being into the lungs of another human being

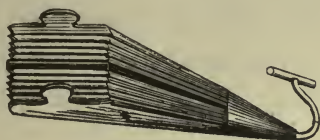
was to commit a grave physiological error, and to intensify the bad condition which was already present. The objection is partly valid, but the method, after all, is not without its advantages. It introduces warm air, and it regulates quantity exceedingly well.

INTERMEDIATE INSUFFLATION—THE BELLOWS METHOD.

A change of method was introduced by John Hunter, who invented a pair of double-acting bellows for artificial respiration. They consisted of bellows with a central partition, giving two cavities ending in a single nozzle. When the bellows were opened one of the cavities filled, through a valve, with pure air from the atmosphere, while the other cavity filled, also through a valve, from the nozzle. In use the nozzle of the bellows was inserted in the nostril of the patient, and then the bellows were worked in the ordinary way. The result was that the air from the lungs of the patient was drawn into one side of the bellows as they were expanded, and common air into the other side. The bellows then contained two charges of air, one impure, the other pure. When the bellows were being closed, the air which had been drawn from the lungs was expelled into the open air, and that which had been drawn from the open air was driven into the lungs. Thus a double current was set up as in ordinary respiration, and the natural process of respiration was imitated.

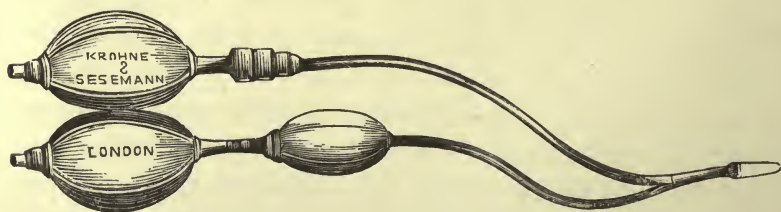
Very early in my experimental career, without being aware, at the time, of Hunter's bellows, I constructed bellows on a similar principle, and have used them until this day, on many occasions. They withdraw and deliver out ten to fifteen cubic inches of gas very readily, but I cannot say they are altogether convenient for the requirements of the practitioner; they take up a considerable

amount of room, and they are rather clumsy in work. The appearance of the bellows is subjoined. They are worked in the same manner as ordinary bellows, and an elastic tube is easily passed over the nozzle to go to the nostril or mouthpiece applied to the patient.



A few years later I invented the double-acting india-rubber bellows for artificial respiration generally known by my name, and which are more practical than the above. They can be carried in the pocket, and are as portable as need be. They consist of two india-rubber hand-bellows, which may be joined together or may be separate, according to choice. The bellows terminate in one common tube, that may be called the nostril tube, and each bellows will contain four cubic inches of air. They are so valved that when, after being compressed together, they are allowed to expand, the one fills with common air and the other, after the nostril tube is inserted in the nostril, with air from the lungs. During compression the air in the bellows containing the pure air is forced into the lung, while the bellows containing impure air is expelled into the open air. When the compression is taken off the bellows fill on both sides, one side from the common air, the other from the lung—that is to say, from the air that ought to be expired by the patient; one side, in short, is supplying whilst the other is exhausting. To the supply side is added a small bag or reservoir, very elastic, in order to prevent undue pressure being made upon the lungs.

In using these bellows the nostril tube is inserted in the nostril on one side, so as to fill the nostril completely ; then the other nostril is closed by pressing with the left finger and thumb, firmly, on the nostril tube, and the bellows are worked with the right hand. If the chest of the subject be small, as, for example, the chest of a newborn child, both bellows may be worked at the same time ; but if the chest be large, the chest, for instance, of an adult man, five consecutive strokes may be made with the bellows which fills the lungs, and then three with that which empties them. In this way natural respiration can be very closely imitated, and the lung can be filled and emptied ten times in the minute without any danger of mechanical injury from the action. I have inserted a diagram of these double-acting elastic bellows in a previous number of the ASCLEPIAD, but I repeat it here, as a necessary part of the present article. They answer exceedingly well, and I have had the most satisfactory results from their use. The disadvantage of them is that they are apt to become hard in structure in course of time, and do not then act so freely as is desirable.



The simplest bellows plan of all, and in my opinion the best of every plan, is the following. Take a piece of soft india-rubber tubing, one quarter of an inch in diameter and two feet long. Into one end of this tube insert a short tube of bone or vulcanite, to make a firm tube for insertion into one nostril ; pass the other end of the elastic

tube to the nozzle of a pair of bellows—hand bellows such as are used for ether spray, or a pair of parlour or kitchen bellows, as may be most handy at the moment. Insert the nostril tube into one of the nostrils of the patient, and, standing on the right side of the patient, take the nostrils in the grip of the left thumb and finger; compress the free nostril on to the tube, and with the right hand, or, better still, by the hands of an assistant, let the chest be gently inflated with air, until it is seen to be filled. At this point remove the pressure from the free nostril, and allow the lungs, by their own elasticity, to empty themselves, assisting the movement, if necessary, by gentle pressure on the abdomen. These acts may be repeated ten times in the minute. The method is very effective, and is not fatiguing; but it has the disadvantage of requiring two persons to carry it out effectively.

THE METHOD OF ELECTRICAL EXCITATION.

I once devoted a considerable time in research, by the experimental method, in order to discover if by electrical excitation, simply or combined with artificial insufflation, natural respiration could be established after it had been suspended. The results were unusually important, although negative in a practical sense. I got Messrs. Krohne and Seseman to make for me a double-acting bellows so connected with a Faradic machine that at each compression of the bellows a current passed. A metal wire from the bellows attended the nostril tube, and formed, very conveniently, one of the electrodes. The other electrode from the battery terminated as a needle, and was connected to a button within the bellows, which could, by compression, be brought into contact with another button connected with the nostril wire. In action, whilst the bellows were filling with air, there was no shock; but

when they were closed, and the lungs were filled, a shock, of any strength desired, was delivered. By introducing the needle from the free electrode into different parts of the chest, into the pectoral muscles, or into the diaphragm, muscular action could be produced, so long as any vitality remained, at the moment the chest was charged with air by the bellows. It was quite remarkable to observe after death by chloroform how well this plan answered in producing inspiration and expiration. Under it a good vesicular murmur could be obtained, when the diaphragm was made to contract, and loud laryngeal sounds were often uttered. Yet the results were always indifferent, from the circumstance that under the electric excitation which called forth the contractions of the diaphragm, the heart was paralysed and the circulation was brought to a stand.

In other experiments I succeeded in setting up good inspiration and expiration through electric excitation alone, *i.e.* without the bellows, by inserting the needle of one electrode into the larynx and the needle of the other electrode into the diaphragm, and by making and breaking contact every ten seconds regularly, under the direction of a metronome. The effects were the same as when the bellows were used with the current; the excitation which called the diaphragm into play produced paralysis of the heart.

VALUE OF ARTIFICIAL RESPIRATION.

Artificial respiration is such a ready practice, and at first sight seems to promise so much, we wonder, after a few years, why such a little comes out of it. I have heard a practitioner of long experience say that he never was sure of having seen any good from it. He had seen what looked like success following upon its use, but never after

the vital irritability of the respiratory muscles had been lost. He adduced, in proof of this statement, a well-known instance that had come under his own observation, in which the captain of a vessel and three of his men, after being capsized in a boat, were carried on board a steamer in the same state of apparent death from submersion. The captain was carried into the cabin, where the surgeon and his assistants used every effort of artificial respiration to restore him to animation, without avail. The three sailors were simply carried down to the engine-room, and there, at the high temperature, they all came rapidly back to life without other assistance from anyone. Something like to this is sometimes seen in experimental work. I remember a lecturer bringing to lecture a kitten that had been drowned, and had been under cold water for three hours, in order to demonstrate to his class some anatomical fact connected with the heart. He had wrapped the little animal in wool, and had put it into the pocket of his under-coat; but when he came to take it out he found it breathing regularly. It recovered, and grew up to become the favourite cat of the household. It is, therefore, not always easy to say if results seen to follow upon artificial respiration are *post hoc* or *propter hoc* the artificial procedure. The only way that I saw open for assurance on this point was to put two animals of the same age and kind into the same anæsthetic chamber, and let them both sink into death; then remove them, lay them on the same table, at the same temperature, and set up artificial respiration in one, whilst the other was left to its fate. This is absolute proving, and it gives an absolute result when it is properly carried out. I have carried it out many times, with the result that life can be restored to the animal that has been subjected to the artificial plan, whilst the animal left to its

fate remains dead. I could make this demonstration at any time, and I have often made it at my lectures on resuscitation. It offers, perhaps, a correct and complete view of the sum total of value of the artificial respiratory process.

And here I may state, at once, that this demonstration teaches several lessons. (1) That if any good is to come from the artificial method it must be set up at once. (2) That so long as there is natural breathing, however slight it may be, it is better than artificial. (3) That when under the artificial method the natural is re-established, it is right at once to let the natural have place, and to cease all further attempt at the artificial so long as the natural is in progress. (4) That until return to the natural act takes place, it is wise to continue the artificial aid so long as the muscles remain flaccid and the blood in the veins remains fluid. (5) That in carrying out the artificial method no force is necessary, and that any extreme force is dangerous to the lungs by imperilling structures that may be easily ruptured if subjected to undue stress or sudden violence.

The reasons for these few observances are of great weight, since success or failure depends on several minor circumstances, which put together make up an important major result. If from the mode of death the heart and the breathing organs have failed simultaneously, the lapse of a very few moments suffices to render the artificial process inoperative for good. Like Hooke, I have seen the motion of the heart restored in inferior animals by inflation of the lungs, but never after the right heart has failed to contract for a period of thirty seconds in both its cavities; so that we may consider this mode of arrest as extremely fatal. Again, when the action of the heart continues on the right side, if the pulmonary

arterioles be contracted, the chances of recovery, under artificial inflation, are extremely small, because it generally happens that before the pulmonary vessels relax the blood contained in the pulmonary tract undergoes coagulation, and thereupon all attempts at re-establishing the pulmonic current is out of the question. Once more; the blood, though it may be in circuit, may not be in a condition to assimilate oxygen, and artificial respiration, however long applied, may then fail. Lastly, in some forms of death, commencing at the lungs, there seems to be a determinate failure of power in the nervous centres to sustain the respiratory movements independently, and then death occurs, even after indications of consciousness have been seen as the result of the artificial method. Thus, as I taught in 1869, the value of artificial respiration is always somewhat limited. To insure its success there must be; (*a*) motion of the heart; (*b*) open pulmonary vessels; (*c*) fluid blood; (*d*) blood that will undergo natural oxidation; and, (*e*) continued vitality of the nervous and muscular systems.

It is essential to submit these difficulties, in order to prevent the wonder and disappointment which are so often felt after failures, upon false promises of success, made on narrow experience and little knowledge. The statements which sometimes are allowed to go the round of the press, and which even find their way into scientific journals, to the effect that a new mode of artificial respiration has been discovered, under which life may be restored in all cases of death from suffocation and from narcotic vapours, if the attempt be only continued for a sufficient length of time, must be taken *cum grano*. It is true that extraordinary success does now and then follow prolonged artificial respiration, but such success is only obtained under specially favourable circumstances, when

every condition leading to recovery is in train for recovery.

THE EXPECTATION FROM ARTIFICIAL RESPIRATION.

Whilst the facts named above are still fresh on the mind, it will be in place to indicate the conditions of sudden dissolution, during which artificial respiration may and may not be expected to prove of service.

ACTIONS OF GASES AND VAPOURS.

We may consider, in the first instance, deaths from inhalation of narcotic and other poisonous gases or vapours.

Gases and vapours capable of arresting respiration on being inhaled may be divided into two classes : one which may be called *negative* in action ; another which may be called, for want of a better term, *positive* or irritant. If I see before me a table of specimens of these substances I can pick out each kind, and can tell, at the same time, those which would allow of recovery under artificial respiration, and those which would not, or, at all events, would allow the least chance of recovery. As a rule, the negative in character are least injurious, and permit the best recoveries by the artificial method. These run as follow, according to my observation of their action and of the effect of artificial respiration after they have caused arrest of vital action by the lungs :—(a) Nitrogen ; (b) hydrogen ; (c) nitrous oxide ; (d) marsh gas ; (e) methylic ether. After these follow next in order : (f) heavy carburetted hydrogen ; (g) vapour of sulphuric ether ; (h) methylal. From suspended life after the inhalation of any one of these substances, recovery may be secured by artificial respiration, as a general rule, if natural respiration has not been suspended longer than from ninety to one hundred and

ten seconds, before the artificial respiration has been commenced.

The reason why artificial respiration is so often successful after apparent death from inhalation of the agents enumerated, arises, I think, from the circumstance that they are all more or less inert. Received into the lungs, and passing, by diffusion, into the blood, where they displace a portion of the gases belonging to the blood, they inflict no more injury than occurs from displacement. They do not stimulate muscular action; they do not cause contraction of the arterioles, either of the pulmonary or of the general arterial system; they produce no disorganization of blood; and they lead to no serious molecular change of the nervous matter. When, therefore, they destroy life the process of destruction is due to changes incident purely to prolonged arrest of function, not to any chemical or physical change excited by their direct action on the structures with which they may come into contact. The immunity from death which occurs so repeatedly, year after year, in the human subject submitted to the influence of nitrous oxide gas, may be traced in this way to the negative action of the gas. The gas does not combine; it does not disorganize; it does no more than arrest the process of respiration and feed the blood for a brief period with an asphyxiating substance.

The success of artificial respiration, even in cases which seem to be the most favourable for its application, is not, however, uniform. The success may be modified by many accompanying circumstances, a fact which can only be correctly learned from experimental observation. I will state some illustrations as they have come before me during experiment. If two animals be submitted to artificial respiration after asphyxia, one with the stomach

loaded with food, the other with the stomach empty, the one with the stomach loaded will not be restored, the other will be, all other circumstances being equally favourable, and the mode of artificial respiration being the same. If two animals be observed, in one of which there has been long deprivation of food, and in the other recent replenishment with time for digestion, the exhausted animal will not respond, whilst the well-fed animal will. If a young animal and an old one be the subjects under observation, it will invariably be found that the young animal will have many chances of recovery over the old one; in fact, a young, though apparently lifeless animal, may not actually be in danger, whilst an old one is hopelessly dead. Again, the temperature and condition of the air at the time of artificial respiration is all-important. At a temperature of 90° Fahr., with the air charged with water vapour to 30° on the hygrometric scale, there will be much greater probability of recovery than with an air at 50° Fahr. with a larger charge of moisture; again, a low and dry will offer a better chance than a medium and moist temperature. But the most important matter to remember of all is that success or failure depends largely on the time that has been expended in inducing the asphyxia. If the asphyxiated condition has been rapidly induced the recovery is easy, if recovery is to follow; if the asphyxia is brought about slowly, recovery is always difficult, and always prolonged. The explanation of this circumstance is simple enough. Under prolonged asphyxia the blood and all the tissues have been brought within the influence of the asphyxiating agent; the muscles are under its influence, and the nervous centres are under its influence; in plain terms, the whole body is under its influence, and, to re-start the breathing is merely to clear the breathing organs, and to wait there. Presuming that

there be a minute current of blood passing over the lungs, from the right to the left side of the heart, that current properly fanned may be a means of escape after a long time of artificial respiration; but this result is uncommon, and is very different from the quick transition from life to death seen after rapid asphyxia, when, although the death may be considered as certain, the recovery is induced, as by magic, under a few strokes of the artificial respiration.

I have been speaking above of negative gases and vapours. When we come to other bodies of the gaseous or vaporious class, which on being inhaled do more than simply replace air, that is to say, which act chemically on the blood, or are freely absorbed and held by the blood, or which, on being carried by the blood into the extreme parts, produce a physiological disturbance in muscular fibre, or in nervous matter, causing thereby a disconnection between nervous and muscular functions, we are on different ground altogether. In these conditions artificial respiration is of little value, except it be commenced immediately after the respiration has ceased.

So much for the value of artificial respiration after suspension of life by inhalation of narcotic vapours. It is necessary, next, to refer to its value after suspension of life from mechanical obstructions to respiration, as after drowning, strangulation, or other forms of mechanical suffocation. In my experimental researches on suspension of life by immersion in water death was found to be remarkably decisive and rapid, except in the case of young animals, the elasticity of whose tissues causes a rapid emptying of the water absorbed into the bronchial cavities so soon as the body is placed in the open air. I have never seen recovery in a *full-grown* animal of any kind as the result

of artificial respiration after five minutes' absolute immersion, with one exception, and that in the human subject. Under five minutes the success of the artificial process is striking, if it be done promptly and correctly. The first thing necessary for success consists in completely emptying the bronchial tubes of the water which they contain; a plan best carried out by lifting up the lower part of the body face downwards, and making pressure on the abdomen and thorax. Until this is done, and the course is made clear for the free and easy entrance and exit of air, the mere act of driving air into the lung is calculated to do more harm than good, from forcing water, like a water wedge, into the air vesicles, causing rupture of the vesicles, with infiltration of water into the cellular tissue and destruction of pulmonary elasticity. When the lungs are free to receive air, then the artificial introduction and extraction of air is the one remedy. But now another element of practice comes into requisition, second only in importance to artificial breathing itself, and that is the introduction of an air well warmed. A temperature of 120° Fahr. is not one degree too high for artificial respiration after drowning; and much of the failure which is met with after the very best method of introducing air rests on carelessness or inability to carry out the process in warm air. Warm air tells usefully in every way. It produces evaporation from the lungs, which is useful for oxidation; it favours evaporation from the skin, which, in its turn, favours circulation; and it restores warmth to the body at large, a great advantage of itself after the deprivation of heat to which the body has been subjected. In all hospitals and receiving-houses for the drowned, a room with a high temperature should at all times be ready for those who have been immersed. In steam vessels the engine-room is the place to which

every person who has undergone immersion should be immediately carried.

When asphyxia has been brought about in the air, as in cases of hanging, strangling, and suffocation, artificial respiration stands at its best as a means of restoring life. Presuming that in apparent death from suffocation no injury has been inflicted on the spinal cord, and no compression on the vessels of the neck, it is astounding to observe what rapid recoveries will take place after every sign of death, following upon mechanical suffocation, has been presented. In some instances the spontaneous return of respiration has been known to occur in criminals after suspension for a time long enough for the legal period enforced to insure the extinction of vitality; and the ease with which, in these and other cases, the body, after a long time of apparent death, can be called back to life, has to be seen to be accredited. In a boy upon whom I performed successfully the operation of tracheotomy for diphtheria, and who was suffocated afterwards from plugging of the tube, life was restored by artificial respiration eleven minutes after natural respiration had entirely ceased. In these instances of pure asphyxia every condition is favourable for artificial respiration, and for recovery under it. The lungs are charged with blood; there is a chance that the current of blood between the right and the left side of the heart is not actually broken, though it may almost have ceased to move; and there is also a fair chance that the blood may not have undergone coagulation.

SUMMARY.

The subjects considered in this essay may be summarized, from what was submitted in last ASCLEPIAD on asphyxia, apnœa, and syncope of the lesser circulation, as follows.

Theoretically.—Asphyxia is the state of suspended animation in which artificial respiration may be expected to be of service. Under apnœa or syncope of the lesser circulation its value is problematical.

Artificial respiration is most likely to be effective, in young subjects, after negative modes of death, and after death rapidly induced.

Practically.—Artificial respiration should always be preceded by means for clearing the trachea and bronchial passages of all obstructions, solid or fluid.

It cannot be commenced too early after natural respiration has ceased.

It should always be carried on, when it is possible, in a warm and moderately dry air.

The best manual mode is, probably, that of Dr. Andrew H. Smith, described at page 208.

The best mode by insufflation, and the best mode altogether, when it can be done, is that by the nostril tube and bellows, described at page 212.

Artificial circulation should be continued so long as the blood in the veins remains fluid, and the muscles respond to electric excitation.

LESSONS FROM THE FASTING MANIA.

*"There is some soul of goodness in things evil,
Would men observingly distil it out."*

—"Henry V."



WHILST the craze about fasting men has been again under discussion, I have looked back on my medical experience to see what knowledge it might afford on the question of fasters and fasting. I find from this review of the past that I have met with two clear examples of death by voluntary fasting. The latest of these is too near the present to allow me to give the details. The other, having occurred so far back as 1848, and having been recorded already in part, may now be rendered in the following report.

A FAST OF FIFTY-FIVE DAYS.

A gentleman, about thirty-three years old, had often been subject to fits of depression and melancholy. He was a man of good social position, had somewhat distinguished himself in his scholastic life, and was always considered as extremely good-natured and thoughtful, though from his earliest age obstinate and self-willed. He was one of those of whom it is said that if "he took anything into his head nothing would turn him." He was not subjected at any time to much restraint; and, as he was comfortably provided for by a business which

demanding but little personal attention, he really had as small occasion for anxiety as most men I have known. He read a great deal, cared nothing for out-door or athletic amusements, and was somewhat listless about the course of events, though he could usually be interested in political controversy, and up to his death was wont to speak on the state of political parties. He was not the only man of his turn of mind, in my experience, who, whilst brooding over his own infirmities, has been inclined to political discussion; but he perhaps showed this tendency more than others of his class. He was always nervous about himself, as I was told, and yet, at the same time, was ready-minded and even courageous in the face of sudden danger. In religion he was not enthusiastic, and his melancholy was untouched by any saddening religious sentiment; but he brooded over imaginary physical evils, which he almost invariably referred to the stomach, and he sought advice from men of all kinds who professed to practise medicine, having just as much faith in a pretentious quack or in the veriest old woman, as in the most regular professor, so long as his whim for liking them lasted. In a word, he became, as his friends said, a confirmed hypochondriac, a man to be pitied, and beyond hope of amendment.

In stature this gentleman was tall, I should say near upon six feet. In figure he was, naturally, very slight, and he was at all times a small eater. To the best of my recollection, he took no wine nor other alcoholic drink; if he took any, it was the smallest quantity; so that, though he would be under no pledge, nor connected with the total-abstinence movement,—which at the time was little considered—he was, practically, a total abstainer.

For many years the condition of this gentleman had continued the same. He was induced to try the effects of

change of air and scene ; but this he declared wearied him too much, and finally he settled down a confirmed invalid of the *malade imaginaire* type, pure and simple. In seeking one day advice from a professor of a schismatic school of physic, he gathered what he supposed to be an entirely new light as to the cause of his malady. The professor, very learned and imposing, detailed to the sufferer the ideas then prevailing as to the cause of primary digestion, from the experiments which Dr. Beaumont had conducted on that most interesting of physiological instructors, Alexis St. Martin. The history of the accidental shot which made St. Martin such a figure in history, the account of the opening into his stomach, and the notes that had been made from visual inspection of the process of digestion ; the description of the gastric juice that was extracted ; and, the further explanation as to the solvent action of the gastric juice on food, became a perfect fascination for the anxious invalid ; and when the learned expositor improved the occasion by telling his patient that all this demonstrative argument was but a prelude to the grand inference he drew as to the patient's condition, the inference being no more nor no less than that the unfortunate patient could not possibly digest food because he produced no gastric juice, the impression produced was positive, and unanswerable.

From that day, by a kind of logical determination which was, I may say at once, impossible to combat, so as to carry conviction to the mind of the sufferer, he maintained that, as he had no gastric juice, it was utterly useless for him to take nutriment of any kind, except water, which required no digestion. The idea implanted in his mind held its place, and was never uprooted. Unfortunately, it was confirmed by the effects of a first attempt at reduction of food. The stomach, no doubt very feeble

and irritable, was relieved by a reduction of food, and therewith the depression of mind was signally relieved, an occurrence by no means unusual, and perhaps a natural consequence.

Soon after his first attempt to reduce food to a minimum, there succeeded another stage, in which the desire for food appeared to pass away altogether. Then when, by a great effort and with much repugnance, food was taken, it caused pain, disturbance, and a greater depression than usual of mental power, with a more determined dislike to the process of feeding, and a firmer and deeper conviction of the truth of the hypothesis that he failed to produce digestive fluid.

In time there seemed to be an entire failure of desire for food ; a loss of sense of taste ; a loathing at the odour of food ; an irritable objection to have the subject of feeding even spoken about ; and, finally, a resolute determination not to take any more food at all unless appetite or desire for some particular kind or quality of food revisited him. From that moment the rigid fasting commenced. Of water he would partake readily, but not largely ; for he said that in quantity it was heavy and cold, and caused painful distension. He would take it to allay thirst, and nothing more. For ten days, under this *régime*, he went about the house, and walked occasionally in the garden, refusing medical advice. After this he took to his bed, and declined to rise except to have the bed made. He now wished for medical attention, but was as resolute with his medical advisers against taking food as he was with the members of his family. Once an effort was made to feed him, perforce, with milk ; but he resisted so determinately, and subjected himself to such danger by his resistance, that the attempt was not made a second time.

A great reduction of bodily weight occurred, in the gentleman whose history I am now detailing, during the earlier stage of the process of fasting. He sank into the extremest state of emaciation during the first three to four weeks of his trial, after which he did not seem to me to undergo rapid change, although I saw him almost daily. He slept a great deal, and at times he tried to read; but the effort at reading soon became wearisome and painful, and was never more than a mere listless occupation. He was not at any time irritable, except when pressed to take food, and he was fond of hearing the current topics of the day; but he soon became weary with conversation, and would drop off into a semi-somnolent state while conversing. I never heard him complain of any pain or discomfort; he did not seem to express or feel desire to live, and he certainly never expressed any desire to die.

As the last days of his life drew near he became much feebler rather suddenly, and his mind, I thought, was inclined to wander for brief intervals. But he quickly recovered himself, and on the day before his death he was unusually clear in his mind. He was painfully shrunken in feature; his voice was low, and almost bleating; his colour was leaden dark; his lips were blue and cold; his limbs were cold; and his breath was cold and offensive, having the odour of new-opened clayey soil. On the morning of his death he, for the first time from the commencement of his fast, expressed that he would eat, and that which he wished for was fruit or raw vegetable, with cream. An attempt was made immediately to pacify his desire, under the hope that, if he once recommenced to take food of one kind, he might be tempted to take more promising support; but it was of no avail, and in fact nothing was swallowed. Soon after this he sank into

unconsciousness, and so succumbed. He died on the fifty-fifth day of his fast, having abstained from all food and partaken of no other drink than water for seven weeks and six days.

I had the opportunity of taking part in the post-mortem examination of this gentleman on the day immediately following upon his death. The emaciation was so extreme that he might almost be said to be a skeleton clothed in semi-transparent flesh. The outline of almost every bone could be traced. On opening the chest the lungs were found collapsed, and so shrunk that they looked like small and half-dried sponges, and divided by the knife rather like soft leather than pulmonary tissue. The heart was reduced to quite half its natural size, was empty of blood in all its cavities, and had its ventricles so attenuated that they resembled auricles rather than ventricles; whilst the auricles were mere shrivelled appendages that could not easily be separated from the ventricles as distinctive structures. The abdominal viscera were attenuated to the last degree; the stomach was reduced to a straight tube, and was with difficulty distinguishable from the duodenum. The intestinal canal was empty through its entire length; it was free of redness, abrasion, or ulceration, but the inner surfaces of the colon and the peritoneal surface presented a few dark spots, melanotic in type. The liver was reduced to half the normal size, and the gall bladder was empty and collapsed. The pancreas and spleen were so reduced in size they could hardly be made out, and the kidneys, although they showed no obvious sign of organic disease, were atrophied quite as much as the liver, and were separated, by shrinkage, from their capsules. The bladder was empty and shrunk.

Not a trace of fatty matter was found at any part,

not even in the orbits. The muscles were flaccid, wasted, dry, and leathery to the touch.

On opening the skull cavity the dura mater was found collapsed, dry, and loose, wanting entirely in tension; the arachnoid and pia mater could not be defined, and the sinuses were empty of blood. The cerebrum and cerebellum, like the other organs, were much shrunk; they were white and firm, resembling the same structures after long immersion in spirit. Between the grey and white matter there was no difference of tint.

The brain, which was dissected very carefully, yielded no obvious trace of acute organic mischief. The bulb of the olfactory nerve was reduced to a line on each side, and the optic nerves were atrophied; as were also the globes of the eyes themselves.

Altogether there was universal atrophy of structure, with dryness of every texture and absence of blood.

I have narrated the above details because they indicate most clearly the length of time during which fasting may be carried on in man under favourable circumstances, and the condition to which the body is reduced by fasting before it ceases to carry vitality.

In the *Transactions* of the Albany Institute for 1830 Dr. McNaughton reported a case of a precisely similar kind in a man named Kelsey, who died from self-starvation on the fifty-third day. Kelsey took more exercise than the patient I have referred to, and died, therefore, a little earlier, or rather existed a little shorter time.

LESSONS.

Bringing these facts to bear on the starvation ordeals which were commenced publicly in America by Dr. Tanner, and which are being continued now in London, we

may assume that a forty or forty-two days' fast with continuance of life is well within the order of natural phenomena, and that the human body has a possible power of endurance from ten to eleven days beyond what has recently been attempted, the extreme limit being fifty-three to fifty-five days. It is right to dwell on this point, because the technical opinion on fasting that will have to be given in our coroners' courts, and in courts of justice, as well as the opinion that will have to be written in our technical and standard works of medical jurisprudence, must in future be considerably modified in many particulars. It has been accepted that, after a certain degree of starvation,—a degree comparatively short, after what is now known,—any act requiring much physical exertion is impossible. A once famous medical jurist, whose lectures were always sound and practical, Dr. Cummin, related that a girl eighteen years of age was confined in the depth of winter in a closed room for twenty-eight days. She had with her a gallon of water, some pieces of bread, amounting to about a quartern loaf, and a mince pie; and she was said to have subsisted on this small quantity of food for the twenty-eight days without fire, and to have ultimately escaped from her prison by breaking down a window-shutter that had been nailed up, getting out of a window on to a roof below, and walking several miles, from Enfield Wash to Aldermanbury. In commenting on this feat, one of our most eminent authorities, Dr. Guy, expressed his disbelief; and he was confirmed in this opinion by two other excellent authorities, Drs. Woodman and Tidy, who considered that while it is possible life might be prolonged, "in all the recorded cases the muscles have become so weak before half the time mentioned, that the sufferers could not even help themselves to water, much less walk this distance."

This opinion bearing on starving persons may apply to persons who would succumb easily ; and it might possibly apply more distinctly to persons who have been subjected to starvation by force rather than to those who permit themselves voluntarily to undergo the infliction ; but we must henceforth so far change the usually accepted canon as to admit a wide range, if I may so express myself, of capacity for starvation amongst the various specimens of human kind. It seems clear that, where the disposition to starve goes with the starving, the powers of endurance are immensely prolonged. Nor is the explanation of this phenomenon peculiar. When the disposition for the starvation is present, when the will goes with the experiment, and when faith, whatever it may be fanned by, keeps hope and courage alive, the chances of continuance of life must be greatly increased. There is then no wasting worry and fever of desire ; there is then none of that corroding fear and dread of death which so materially—I use the term in its physical meaning—favour dissolution.

Thus we would expect that men or women who voluntarily submit to starvation, and that men and women who in days of enforced starvation have most courage to endure, will endure the longest, and will recover, if the chances of recovery be offered, with the greatest facility.

SUSTAINING POWER OF WATER.

A second lesson is that life may be long sustained by water alone, and that, in instances where a long period of existence is maintained on mere aqueous fluids, it is the water that sustains. In short, in a sense, water becomes a food. The knowledge of this truth is corrective of some of the most grievous and mischievous errors. Persons undergoing severe privation and fatigue, persons suffering from disease, persons suffering from repugnant dislike to

animal and vegetable foods, have for long seasons been supplied with drinks of wine or of spirits and water. Forgetting the water altogether, or treating it as a thing of no consideration, they have declared—and others, even medical men, have declared for them—that they were sustained on alcohol, and therefore the alcohol was a food. It was vain to indicate that in such cases the alcohol was largely diluted with water. It was vain to urge that the Welsh miners who, some years ago, were buried alive without solid food, were able to live ten days on water alone. It wanted such proofs as these we have now got to demonstrate the actual nature of the sustaining agent, and to exclude the agent alcohol,—which, often obtaining all the credit, does more evil than good.

TREATMENT OF THE FAMINE-STRICKEN.

A third lesson relates to the practice of treating the starved in times of great famine, and to the treatment of districts where famines most commonly prevail. Mr. Cornish, in his admirable Report on a great famine in India, takes the utmost care to explain that the danger of the deficient food supply was comparatively small when there was any sufficient quantity of moisture. So long as fruits and herbs and plants of a succulent and wholesome kind could be obtained, so long there was strictly no famine. But when the juices of fruits and other succulent vegetable supplies of water were cut off, then indeed the people were famine-stricken with a vengeance. Mr. Cornish also refers to another fact—briefly, it is true, yet still with sufficient effect to show his meaning—that when the famine-stricken had passed a certain period of time without food or drink, when they had to a large extent lost the desire for food and drink, they frequently died even when the relief came and food was carefully supplied

to them. He relates that in one instance he took a sufferer to his own home, and there, with the most scrupulous care, tried to restore life and health, but without avail; and he is led to explain that there is a period in a famine when all the foods that may come in are practically useless to the persons who are in hunger and athirst, and yet do not at first sight appear likely to die. This is the secondary effect of famine on the body; but, be it observed, it only occurs when, in addition to deprivation of solid food, there is also deprivation of fluid. Let the fluid be supplied in even small quantity, and, though the emaciation may be extreme, death may be averted, and the subjection of the stomach to new and proper aliment may lead to perfect restoration of life.

In famine, therefore, it is a first point of practice to supply water to the sufferers, but in a fair but moderate degree, for the water itself may be supplied more freely than is proper, and danger may thereby be promoted.

The lesson respecting famine extends from the particular to the general. It passes from the physician to the statesman. Cornelius Walford, in his valuable essay on the famines of the world, past and present, teaches that while solar heat, combined with moisture, affords the most certain means of securing luxuriance, it causes, without the moisture, a howling wilderness. The fact is evidenced in India, where, under irrigation of land, without luxuriant vegetation to defend the earth, there is, even in the presence of water, a howling wilderness and a district for famine, as if the earth itself had lost its power to live and reproduce when the famine of drought came upon it. The lesson taught is, that to prevent districts of famine the sources of natural moisture for mother Earth herself must be kept up, so that, though she may be deprived of carboniferous and nitrogenous food, she may revivify

when the normal conditions of life are restored. Even with the earth the supply of water must be gentle and moderate. Flood it, and it is destroyed; but the life that it holds, deprived of due supplies, will live after long deprivation, and will be renewed, in all its luxuriance, if it be re-fed with natural and proper care.

In cases of sudden accident, as in our coal-mines, when living human beings are buried away and given up for lost, it is now plainly, I had almost said authoritatively, suggested to us that more prolonged search should be made for those that are lost than is often thought necessary. The miners who, after ten days' immurement, were at last rescued, might have lived many days more,—twice ten, almost certainly,—and yet it is to be feared they would have been given up long before the first ten days had elapsed, had not sounds from them reached the ears of those who were in search. In discovery for those who are immured a search extending even to forty days would not, as we now know, be needlessly long. Moreover, it would be useful for those who are exposed to such dangers as are now under notice, to be instructed in the truth that, if they can obtain water, and will trust to it to the exclusion of all spirituous poisons, they may expect to live in a natural air for a period varying from three to six weeks. Such knowledge would give both hope and fortitude to unfortunates who might, otherwise, be led to any rashness of despair, and might open many chances which would not occur to them in ignorance of the light that has now been thrown on the subject of human endurance under privation from solid food.

LESSONS IN ECONOMY.

Fourthly, a lesson is rendered to economic science. When we know how little food is really required to sustain

life we may the more readily surmise how very much more food is taken by most persons than can ever be applied usefully towards sustainment. I have no compunction in expressing that, while fasting enthusiasts are subjecting themselves to considerable danger from abstinence, hundreds of thousands of persons are subjecting themselves to a slower but equal danger from excesses of foods and drinks. These keep up their experiment, and, with every vessel in their bodies strained to repletion and seriously overtaxed, continue to replete and to strain the more. If we could induce, therefore, such persons to contemplate their proceedings, and to strike a fair comparison between their own foolhardiness and that of the faster, the moral they would easily draw would not be without its worth. Unfortunately, the comparison cannot be made with effect, because the feat of excess is in the swim of fashion, while the feat of fasting is very much out of it. The first is a vice which, by familiarity, begets favour and competition; the second is a folly which, by its oddity, begets only curiosity, compassion, and contempt.

PHYSIOLOGICAL LESSONS.

In a physiological point of view, a good many lessons are to be learned from the fasting mania. That during a fast of forty days the temperature of a man should to the end remain steady is of itself an important bit of evidence. We have been led to believe that in a very few days the process of abstaining from a sufficient supply of food, to say nothing about abstaining from food altogether, is a certain means of reducing the animal temperature. It was never surmised that water alone would lead to conditions in which the vital warmth would for many weeks remain practically sustained. That the respiration should remain so little affected is a second equally remarkable

fact; and that the muscular power should be kept up so as to enable a starved man to walk, talk, and compress the dynamometer to 82° for forty days is beyond what any physiologist living would have admitted as possible previously to the events that declares the possibility. These results, coupled with unquestionable waste of tissue, and with the painful and frequent disturbance of the stomach, are quite sufficiently remarkable to demand the attention of the thoughtful physiological scholar.

The most striking physical lesson of all remains, namely, that during the whole of the fasting period the mind of the faster is unclouded, and, taking it all in all, his reasoning powers are good. Whoever remembers what depressions of mind, what lapses of memory, what stages of indecision and vacuity come on when for a few hours only the body is deprived of food, will wonder, not a little, that any human being could remain self-possessed and ready for argument and contention during a fast of over six weeks. Yet from the examples supplied, the possession of mental is even more conspicuous than that of physical endurance. Suppose it be urged that the excellent sleeping faculties of the fasters kept their minds in good balance, we do but move the difficulty one step farther back, since to sleep in a state of fast, and to wake again refreshed, is itself a strange order of phenomenon. In sleep there is in progress the repair of the body. How shall there be repair when the food material out of which the repair is secured is not supplied? For a starving man to sleep and die we might be prepared; for a starving man to awake in the shadow of semi-consciousness or dementia, or for a starving man to wake in the terror and excitement of delirium and rage, we might be prepared; but for such a man to wake up refreshed and, at the worst, no more than irritable, is a new revelation affording un-

suspected evidence of the grand part which water plays in the economy of life. The physiologist, himself, will wonder how water sustains life for such long periods. He will see that under its influence a kind of peripheral digestion is established in the body itself, by which, independently of the stomach, the body can subsist for a long time on itself; first on its stored-up or reserve structures, and afterwards on its own active structures. He will infer that, by the influence of the water imbibed, the digestive juices of the stomach are kept from acting on the walls of the stomach. He will discern that, by the steady introduction of water into the blood, the blood corpuscles are retained in a state of vitality, and in a condition fitted for the absorption of oxygen from the air. He will note that the minute vesicular structures of the lungs and of all the glandular organs are kept also vitalised and physically capable of function; and he will understand how that water-engine, the brain, is sustained in activity; its cement fluid, and its cell structures free.

The act of the professional faster, of taking some undescribed powder as a sustainment, is, in my opinion, either a self-delusion or a pretence. It may, as a fancy or placebo, give faith, support the mind, and strengthen the will; or it may be a mere pretentious discovery. Whichever it be, the evidence is certain that the ordeal can be borne without it by those who can undertake the ordeal, a class of men who are specially constituted to starve, and who, by the speciality, are led to undertake what to the ordinary constitution would be impossible and which under compulsion would often end in death in the second quarter of a trial of forty days.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

ACUTE POISONING BY CHLORAL HYDRATE.

WITHIN two months after the delivery of my Report on Chloral Hydrate to the British Association at Exeter, in 1869, in which report the remarkable effects of this hydrate were first made known in England from the discoveries of Leibrich, I was called by the late Dr. Whitmore, of Harley Street, to a gentleman who, either by intention or accident, had taken an overdose of chloral, and who was lying in a state of complete insensibility, bordering on death. It was the first instance of poisoning by chloral hydrate that had occurred in England, and, as far as I know, in the world. An immense number of suggestions were offered in respect to the treatment to be pursued, but I was resolute in enforcing one course alone. I had observed, in watching the action of chloral hydrate in large doses on warm-blooded inferior animals, that death never took place until fluid secretion was freely thrown out into the bronchial passages, producing what I have designated *hydrops bronchialis*. I had further observed that *hydrops bronchialis* could be prevented by the simple process of keeping the narcotized animal in a high temperature. In the case in hand the first note of commencing

water obstruction in the bronchial passages was showing itself by a moist râle; but the case was not hopeless. A large fire was lighted and kept up; bottles of hot water were placed all round the body within the bed, the warmed air from which was allowed to escape freely over the face; and sulphuric acid, in open beakers, was set in different parts of the room to absorb water from the atmosphere. The patient was subjected to no other treatment, except that he was fed with milk through a tube passed into the stomach by the nostril. The lungs cleared quickly, and after six hours' sleep recovery commenced, and continued to completion, thirty-one hours after the poisonous dose, unmeasured, had been swallowed.

Three years after this event I was called to another similar case, in which a patient was rendered insensible from a dose of the narcotic of not less than 240 grains. The subject of this accident was a middle-aged man of full habit and vigorous constitution. He had suffered from neuralgic pains, and had been also subjected to severe mental labour, from professional work. From time to time he had taken chloral as an anodyne, and could bear a dose of from forty to sixty grains with what he considered benefit rather than injury. During a few days of unusual fatigue and worry he remained quite sleepless all through the night, and after two restless nights commenced early on the morning of the coming day, during which the symptoms had continued, to take chloral. His first dose was fifty grains, and as sleep did not follow it so soon as was desired he took twenty grains speedily after, certainly within the hour. A bottle containing the remainder, nearly half an ounce, was in his room; and a little later, while in a semi-conscious state, he mixed the whole of the contents of this bottle with water and swallowed the draught. For several hours afterwards he was sup-

posed to be simply sleeping, the repose was so placid and so perfect. But at length the symptoms took a more serious turn; it was found that he could not possibly be roused, and his breathing became stertorous. Two hours after this, and nine hours after the last dose of the chloral had been taken, the patient came under my observation. The body was flaccid and the limbs were easily moved, remaining in the state in which they were left, as is seen in catalepsy. The temperature of the body in the mouth was 98° Fahr., in the axilla 97°; over some parts of the surface of the body there was an inclination to perspire, and on the brow there was at times a free exudation of water, the water standing out in large drops. The face was intensely flushed, the ears carmine red, and redness of skin was, indeed, more or less perceptible over the whole of the body. When the skin was firmly pressed with the finger over parts where there was resistance of bone beneath, the blood, on removing the pressure, rushed into the part, giving a peculiarly well-defined, deep red mark, which lasted from three to four minutes. The pupils were dilated and refused to contract even under the influence of a strong light. The body over its entire surface was insensible to impressions, and the senses were equally deadened. The coma was deep, as in apoplexy, and the breathing was stertorous but not blowing. No efforts could arouse the sleeper. The odour of the breath was faintly ethereal, somewhat of the odour of methylic ether; the breathing, though stertorous, was regular, at twenty-eight per minute, and the auscultatory sounds were clear, except that in the regions of the larger bronchial tracts there was, at intervals, a faint mucous râle. The pulse was soft, but full and steady, giving sixty-four strokes per minute. The sounds of the heart were normal, but occasionally there was slight intermission in the stroke.

These were the leading symptoms in this case; they were sufficiently clear to supply the diagnosis of chloral narcotism before the facts about the administration of the agent were related. The treatment followed was that I originally suggested for such cases. The danger of death being from condensation of fluid in the minute bronchial canals, followed by decrease of animal temperature, the first point insisted upon was to sustain, as before, by every available means, the already failing animal heat. The bladder was watched, that it should not become distended with urine, and warm food was conveyed into the stomach while still the patient was unconscious. After seven hours, signs of restless movement indicated rallying from the lethargy, and soon afterwards the sleeper, though he knew nothing of the fact subsequently, rose, with assistance, and passed urine. Then he swallowed drink and again sank into deep slumber, but without stertor. Six hours later he again awoke more conscious; he passed urine and took fluid food. He reclined once again to sleep, and on the following visit, next day, twenty-six hours after partaking of the narcotic potion, he was still drowsy and disinclined to move, but was conscious and capable of helping himself to move and to swallow. He had no remembrance of the events that had occurred, or of the time that had elapsed. He made a rapid recovery.

SYMPTOMS OF POISONING BY CHLORAL HYDRATE, ACUTE AND CHRONIC.

THE symptoms of acute poisoning by chloral hydrate are, briefly, profound coma, great muscular relaxation, apoplectic breathing, and flushing of the face and neck, with intermissions of pallor. The eyes are usually rolled upwards,

as under chloroform, and at intervals, as impressions of motion are made on the surface of the body, there is muscular tremor, which may pass into convulsion. In time the extremities become cold, and the bronchial surfaces become charged with frothy mucus, which greatly impedes the breathing and hastens the final result—hydrops bronchialis.

The chronic symptoms of chloral poisoning are ; sleeplessness, unless the narcotic be taken in very large doses ; great mental irritability and muscular prostration ; uncertainty of movement, with tendency to fall forward ; caprice of appetite and frequent nausea. In some cases there is injection of the conjunctivæ, and in other cases yellowness. The urine, in extreme cases, contains albumen, and the bowels are commonly constipated, the evacuations being white and hard. Chloral hydrate does not produce the ecstatic dream or delirium caused by opium or haschish ; on the contrary, it causes, through all the stages of its action, a sense rather of depression than of elevation of mental faculty.

Continued for a long time, the symptoms from chloral approach very near to those of purpura, without, however, being attended with loss of blood. The body becomes emaciated ; the breath becomes not only ethereal, but heavy, and almost offensive ; and the surface of the body becomes covered, on the back and on the fore part of the chest especially, with patches of a purpuric rash, the spots of which, large as the ordinary purpuric, are, however, not so dark, but are reddish-brown in colour, approaching somewhat to the copper hue of the syphilitic eruption. On withdrawal of the chloral the rash, I believe, always subsides after a time, but I have known six weeks elapse before it has disappeared.

The craving for chloral after it has been withdrawn is

neither so intense nor so lasting as that for alcohol, opium, and morphia, and no such terrible symptoms occur as those witnessed on the withdrawal of alcohol, opium, or morphia. The reason of this difference arises, I have no doubt, from the circumstance that the chloral hydrate is, comparatively, an unstable compound. It is decomposed in the body into chloroform and formate of soda, each of which is disposed of readily; the first as a diffusible vapour, the second as a diffusible salt which quickly dialyses by the kidney. Hence there is no firm combination of a foreign organic substance with the nervous structures, no condensation, and no new combination.

ADDITIONAL STUDIES ON CHLORAL HYDRATE.

ONCE endeavoured by experimental research to ascertain what is a dangerous and what a fatal dose of chloral hydrate. The conclusion at which I arrived was, that the maximum quantity of the hydrate that can be borne, at one dose, bears some proportion to the weight of the animal subjected to its influence. The rule, however, does not extend equally to animals of any and every class. The proportion is practically the same in the same classes, but there is no actual universality of rule. A mouse weighing from three-quarters of an ounce to an ounce could be put to sleep by one quarter of a grain of the hydrate, and could be killed by a grain. A pigeon weighing twelve ounces could be put to sleep by two grains of the hydrate, and could be killed by five grains. A guinea-pig weighing sixteen ounces could be put into deep sleep by two grains, and into fatal sleep by five grains. A rabbit weighing eighty-eight ounces could be thrown by thirty grains into deep sleep, and by sixty grains into fatal sleep.

The human subject, weighing from one hundred and twenty to one hundred and forty pounds, can be made to pass into deep sleep by ninety grains, and by one hundred and forty grains into a sleep that will be dangerous.

I was led at first to infer that in the human subject one hundred and forty grains should be accepted as dangerous, and two hundred and eighty as a rapidly fatal dose; but Dr. Hills, of the Thorpe Asylum, Norwich, favoured me in 1872 with the facts of a case in which a suicidal woman took no less than *four hundred and seventy-two grains* of the hydrate dissolved in sixteen ounces of water, and actually did not die for thirty-three hours. Such a fact, ably observed as it was, is startling; but it does not militate against the rule that one hundred and forty grains is the maximum quantity that should be administered at one dose to the human subject.

A second point to which my attention has been directed is, what quantity of hydrate of chloral can be taken with safety at given intervals for a given period of time, say of twenty-four hours. To arrive at some fair conclusion on this subject I calculated from a series of observations the time required for the development of symptoms from different doses of the hydrate, the full period of the symptoms, and the time when they had entirely passed away. Great difficulties attended this line of investigation; but I may state, as a near approximation to the truth, that an adult person who has taken chloral hydrate in sufficient quantity to be influenced by it, disposes of it at the rate of about seven grains per hour. In repeated doses, the hydrate of chloral might therefore be given at the rate of twelve grains every two hours for twenty-four hours, with less danger than would occur from giving twelve times twelve (144) grains at once. I do not think this amount ought to be exceeded, even in divided quantities.

I have referred above to the means to be adopted when, from accident or other cause, a large and fatal dose of chloral hydrate has been administered. But I may add a few lines more. It should be remembered that this hydrate, from its great solubility, is rapidly diffused through all the organism. It is in vain, consequently, to attempt its direct removal by any extreme measures after it has fairly taken effect. In other words, the animal or person under chloral must go through a distinct series of stages on the way to recovery or death; and these stages will be long or short, slightly dangerous or intensely dangerous, all but fatal or actually fatal, according to the conditions by which the body is surrounded. One of the first and marked effects of the chloral is reduction of the animal temperature; and when an animal is deeply under the influence of the agent, in the fourth degree of narcotism of Snow, the temperature of its body, unless the external warmth be carefully sustained, will quickly descend seven and even eight degrees below the natural standard. Such reduction of temperature is itself a source of danger; it allows condensation of fluid on the bronchial pulmonary surface, and so induces asphyxia, and it indicates a period when the convulsion of cold, which sharply precedes death, is at hand.

I offer these explanations in order to indicate the first favourable condition for the recovery of an animal or man from the effects of an extreme dose of chloral hydrate. It is essential that the body of the animal be kept warm, and not merely so, but that the air inspired by the animal be of high temperature. This fact is well illustrated by experiment on the inferior animals. In the pigeon an air of 95° Fahr. is most favourable, in the rabbit an air at 105° to 110°, in the dog the same. In man the air to be breathed should be raised to and sustained at 90° Fahr.

The next thing to be remembered in the recovery of persons under the fatal influence of chloral hydrate is to sustain the body by food. I found that even under deep sleep from the narcotic, although the process of waste is less than is common under natural conditions of rest, there is still a very considerable waste in progress, which, if not made up, is against recovery. I found, also, that the digestive and assimilating powers, though impaired during sleep from chloral, are not arrested, but may be called into fair action with so much advantage, that if two animals be cast into deep sleep by an excessive quantity of the narcotic, and one be left without food and the other be artificially fed on warm food, one-fourth of the chance of recovery is given to the animal that is supplied with food. In the human subject warm milk, to which a little lime-water may be added, is the best food. Milk is easily passed into the stomach, by the nostril tube, in portions of half a pint every two hours.

In extreme cases where natural respiration fails artificial respiration should be followed. In all cases the bladder should often be emptied.



THE SICK WARD AS A METEOROLOGICAL STATION.

I CANNOT express how deeply I have regretted, during the past few months, that I have had no opportunities of observing, on a scale sufficiently large, the effects of atmospheric changes on health and disease. The most curious coincidences, as I suppose we must in our ignorance call them, have taken place under my observation, only to remain unexplained. On one special Monday morning three patients came into the consulting-room suffering from nasal catarrh, soreness

of the throat, and acute pain in the muscles of the left shoulder. They all said that a chill contracted on the previous Saturday, in the latter part of the day, was the cause of the symptoms, but they could not tell in what manner they got the chill. One had never left the house all the day; the second had passed through a moderate day of usual work, without any exposure to draught or to fatigue; and the third had had a railway journey, but without being able to recall any particular reason for "taking cold." On that same day the weather record in the after part of the day tells: "barometer falling, moisture of atmosphere increasing, temperature rising." Are such phenomena as these purely and simply coincidental, or do they stand in any relationship of cause and effect? It is impossible to say, but it is quite correct to say that such phenomena require to be looked into, and that, like mites in science, the records of them would, of necessity, yield important results. Here is an open field for labour, abundant for a lifetime of labour. Immediately, there are three subjects for study. (1) The changes incident to atmospheric oxygen under various conditions of heat and electricity; (2) the changes taking place in the nitrogen of the atmosphere; (3) the changes taking place in the water vapour of the atmosphere. Something, though very little, has been done by myself and others in relation to the atmospheric oxygen; but as to the nitrogen and the water vapour all is blank.

In the presence of ozone in the atmosphere is the hydrogen of the water vapour in the same condition of stability as it is when the oxygen is neutral? Is nitrogen always in the same state whatever the electric state? Why in some seasons is there a greater absorption of nitrogen in respiration than at other seasons? Does an increased absorption of nitrogen, in respiration, modify

physiological function? How far is the elimination of carbonic acid modified by atmospherical changes? How far is disease—acute and chronic—relieved or intensified by atmospherical variations? One other question, perhaps the most difficult of all. We know that when what is commonly called “treacherous weather” prevails, persons are, to use an everyday expression, “caught by it,” even when they are indoors or actually in bed. What is the cause of this? What is the subtle atmospheric influence which keeps a patient with lung affection awake all through a night with cough, and permits fair sleep and freedom from cough another night? There is as yet no ghost of an answer to that question, although a true and sufficient answer to it would clear away some of the most serious difficulties which now beset us in our daily course of practice and observation.

Enough has been said to indicate the necessity for new research. But where is research to begin? The centre for the commencement seems to me to be obvious enough: it should be in the hospital ward. In the ward of every hospital there should be set up one of the meteorological cabinets which Messrs. Horne and Thornevaite made for me thirty years ago. This would yield records of atmospheric pressure, of moisture, of temperature, and of presence or absence of ozone. To it should be added a wind gauge, showing the direction and force of the wind. An educated nurse could be taught to take and record all these observations; and, under an army of nurses employed in making such records, without drawing, for the time, any premature conclusions, the sick ward, as a meteorological station, would, in a few years, be one of the most important series of centres of observation that ever the advancing profession of medicine has held in its grasp.

THE DIAPHANOUS TEST OF DEATH.



PARAGRAPH making the round of the scientific and general press must be accepted *cum grano*. In this paragraph it stated that the French Academy of Sciences ten or fifteen years ago offered a prize of £1,600 for the discovery of some means by which even the inexperienced might at once determine whether in a given case death had or had not ensued. A physician obtained the prize. He had discovered the following well-known phenomenon. If the hand of the suspected dead person be held towards a candle or other artificial light with the fingers extended and one touching the other, and one looks through the spaces between the fingers towards the light, there appears a scarlet red colour where the fingers touch each other, due to the blood still circulating, and showing itself through the tissues, if life have not yet ceased. When life is entirely extinct the phenomenon of scarlet space between the fingers at once ceases. The most thorough trials, it was said, had established the truth of this observation. In my essay on absolute proofs of death I have described this test with the others, and have attached to it its true value. The statement that the test is sufficient of itself is, however, too solemn to be allowed to go without correction; and I therefore affirm, with all possible earnestness, that the test, trusted to alone, is capable of producing the most serious error. In the case of a person in a state of syncope, where the test was most carefully applied, there was not the faintest trace of red colouration between the fingers; yet recovery from the syncope was quite satisfactory without any artificial aid. The test is one which admits of being readily tried, and, *primâ facie*, it is a good test to bring into operation. But as an absolute

proof of death I should put before it: (1) the pulsation of the heart; (2) the respiratory murmur; (3) pressure on veins; (4) the electric test for muscular irritability; (5) the ammonia hypodermic test; (6) coagulation of blood in the veins; (7) rigor mortis; and (8) decomposition.

LOCAL ANÆSTHETIC SPRAYS—CARBOLIC ETHER SPRAY.

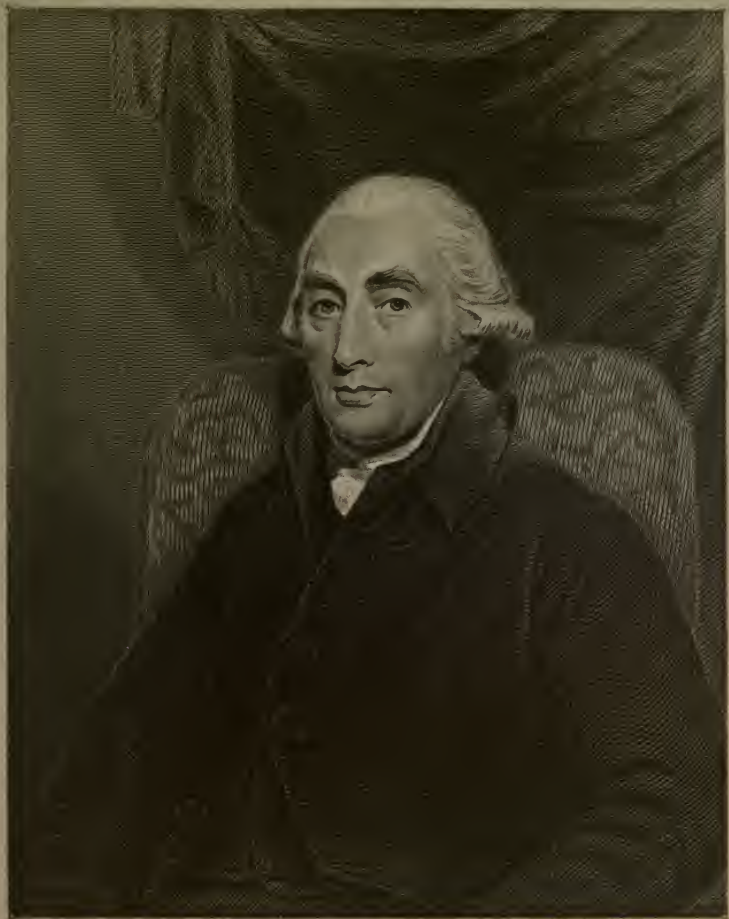
DR. A. DOBISCH of Zwittau has been making some experiments in local anæsthesia with ether spray, using instead of simple anhydrous ether a fluid mixture, consisting of ten parts of chloroform, fifteen parts of ether, and one part of menthol. In this Dr. Dobisch has merely been going over ground which I traversed, over and over again, when I was working, originally, at ether spray. The essence all through is connected with the cold that is produced, and the cold is the anæsthetic. The chloroform and the menthol add nothing to the result. One or two substances do, however, render assistance in a compound spray, and one notably, I mean carbolic acid. Carbolic acid in ether in the proportion of five grains of the acid in five fluid ounces of anhydrous ether yields a good anæsthetic spray, which has its advantages and its disadvantages over simple ether. The advantages are. (1) That the superficial anæsthesia produced appears before the skin becomes hardened from the cold, whereby, in cutting operations, the knife is less impeded than it usually is when there is dense freezing of cuticle. (2) That if deep cutting be necessary a continuance of the anæsthetic spray on the divided part causes very profound anæsthesia, so that dissection of a part can be carried out with continued painlessness. (3) That whatever antiseptic value there may be in carbolic

acid is exceptionally applied by this method. (4) That the anæsthesia induced is prolonged, and pain from reaction, which sometimes occurs after mere freezing, is reduced or prevented. The disadvantages of the carbolic ether spray are. (1) It produces a disagreeable odour; (2)—And this is the greatest disadvantage—it sometimes leaves a wound that does not readily heal, but remains irritable for several days, closes by granulation, and gives rise to a conspicuous scar.—I carry such a scar on my own left hand as a result of an experiment with carbolic ether spray made fifteen years ago. (3) The presence of the acid has a tendency to block and destroy the spray tube if it be metallic, and to block it up if it be either of glass or ebonite. (4) If much of the solution be used, some carbolic acid will be left in the wound, and there may be risk from the absorption.

These advantages and these disadvantages of the carbolic ether anæsthetic spray fully and fairly accepted, it may be of use for the surgeons to give it a better trial than I have been able to carry out; and in one class of local affection I can recommend it as of signal service. I refer to ulcerating cancer, where, with much pain and discharge, there is fœtor. In these cases the fluid can be distributed over the part from a toy or toilet spray-bottle, which costs but a few pence, and one of which will last in use for several days. A few sprays of the fluid are sufficient to give the relief required, and the effect of the benumbing is long-continued.

JOSEPH BLACK, M.D., AND THE SCHOOL
OF CHEMICAL MEDICINE.

IN every scientific coterie the name of Dr. Joseph Black has a familiar sound ; but it is usually the sound of a name, as of a great and mighty wind, which comes few know where from, and goes few know where. Frequently the name is associated with stories of the eccentricities of men of science. At a dinner table of learned men I was once informed, by an aged man of that modern Athens in which Black lived and died, that he (Black) was accustomed to walk about the town in boots made of tin. Further, the same authority stated, as a matter of fact, which some, whom he himself knew, could remember, that Dr. Black was so given to self-absorption and abstraction, that his wife, in company with friends, would meet him in the street in one of his fits of abstraction, and, accosting him, would inquire after Mrs. Black and the family, with other family interrogations, and having obtained satisfactory replies would permit the entranced philosopher to go on with his meditations, without for a moment recognizing who it was that had broken into them. The story was so well told we all accepted it as *bonâ fide* ; nor did I, for my part, discredit it, nor fail sometimes to repeat it, until the fact was revealed to me, on indisputable evidence, that Dr. Black never had a wife, but lived and died one of the



Asclepiad No. 27 Vol. VII.

London, July 1890.

Joseph Black

*Autotype from an engraving by
C. Cook, after a painting by Raeburn.*

B.M.R.

most retiring of old bachelors that ever was known in the city of his adoption. Perchance, therefore, the story of the boots of tin is of the same shadowy nature as that about abstraction, although it does seem that he was a retired and thoughtful man, a man always thinking, and laying his thoughts by to grow and fructify under the light of an ever-widening and increasing knowledge.

Joseph Black was born in the same year as John Hunter, 1728, and, like that remarkable man, was of Scotch parentage on both sides of his family. His father, John Black, was a member of a family from Scotland that had settled in Belfast; his mother was a Miss Gordon, a daughter of a Mr. Robert Gordon, of the family of Hilhead in Aberdeenshire, and her aunt was the mother of the illustrious Adam Ferguson. Mr. Black, father of our philosopher, went into the business of a wine merchant, was led, by business, into the South of France, and took up his residence at Bordeaux during the time when the grand Montesquieu was President of the Parliament of that famous place. He had a large family there, a prosperous business, and an excellent social position, as appears clearly from the following note, written by his distinguished son whose career is now under review, in respect to his parents and his birthplace, Bordeaux.

“My father was honoured with President Montesquieu’s friendship, on account of his good character and his virtues. He had no ambition to be rich, but was cheerful and contented, benevolent and liberal-minded, industrious and prudent in business, of the strictest purity and honour, very temperate and regular in his manner of life. He and my mother, who was equally domestic, educated thirteen of their children—eight sons and five daughters—who all grew up to be men and women, and settled in different places. My mother taught her children to read.

English, there being no school for that purpose in Bordeaux."

The circumstances under which the children of Mr. Black were placed, in relation to their education, led him to send them away from home at an early age; and at twelve years, namely, in 1740, Joseph was sent to Belfast to a grammar school there, where he remained until the year 1746, when he proceeded for a collegiate education to the University of Glasgow. At this time, natural science was just beginning to show its modest face in the Universities, and to science, in its physical aspects, Joseph Black turned his mind. Dr. Robert Dick was the Professor of Natural Philosophy, and Black became his favourite pupil. Black also became the friend and companion of a son of Professor Dick, who, on his father's demise in 1751, succeeded to the chair which he adorned until 1757, when he died of fever.

Left by his father to make a choice of a profession, Black chose that of medicine, not foreseeing, says his friend and biographer, Dr. Robison, during the gaiety of his youth, how much he would suffer by anxious solicitude and fears in the practice of this noble art. His medical studies were entered upon under the direction and observation of Cullen, whose life was presented to the readers of the ASCLEPIAD in the last number, and who at this period was intent on chemistry, as the science of sciences to be linked to medicine. In this intent, Black followed his master with the warmest enthusiasm, and soon became his assistant in the experimental part; for his genius for experiment showed itself from the first, and professor and pupil became fellow-students as well as fast and abiding friends.

The first original researches in which Black was engaged were carried on, as Robison computes, in the years

1751-2, and had reference to magnesia and lime. The medicines then in use for the solution of calcareous concretions, and for stone in the bladder, were the so-called "caustic medicines," of which quicklime was the base. The causticity of the lime was assumed to be derived from the fire in which it had been burned, and this causticity, it was thought, could be conveyed by it to other bodies of an alkaline character. But when lime is exposed to the air it ceases to be caustic, because it has lost by the exposure its igneous property. The view was natural, and at first Black not only accepted it, but proposed to find out some mode by which he could entrap or catch the igneous part as it escaped. Possibly he tried the experiment and found it fail, for soon afterwards he made a note to the effect that "nothing escapes from caustic lime, but that such lime absorbs air." He weighed an ounce of chalk, calcined it, and weighed it again, putting down the loss that had been sustained. Again, he took another ounce of chalk, and after treating it with spirit of salt, collected the residue when the effervescence had ceased, and computed the loss of weight so obtained with the loss from calcination. Step by step he came to the discovery that lime, in its caustic state, imparts nothing to other alkalies, but that it has the property of removing from them the air which rendered them mild. But then it is itself no longer lime, for it effervesces, and good lime does not effervesce. Hence the inference that the causticity of alkalies is the natural property of those substances, and is not igneous. The alkalies do not communicate any igneous property to other substances, but they lose the primitive character belonging to them by absorbing something from the air. A cubic inch of marble is made up of about half its weight of pure lime, with as much gas or air as would fill a vessel holding six

wine gallons. The lime, in combination with the air, is rendered tasteless and mild, as oil of vitriol is rendered tasteless and mild, in the form of alabaster, by treating it with calcareous earth.

The studies which led to these remarkable results were probably commenced in Glasgow, in the laboratory of Cullen; but they were brought to their completion in Edinburgh, whither Black had moved in the year 1750-51, in order to carry out his medical studies, and where he took up residence with James Rússett, the Professor of Natural Philosophy in the University. The position was in all senses fortunate, and was remembered by Dr. Black, with much satisfaction, to the end of his life.

Black made the studies he had followed in chemistry the subject of his inaugural thesis previous to his graduation in medicine in 1754. This thesis was dedicated to Cullen by its author, to whom also he addressed a letter on January 3rd, 1754, giving a new and singular bit of history on one of his experiments with what he then called fixed air—carbonic acid gas. In this letter he apologises to Cullen for not writing because of an experiment he was trying which amused him. "I had mixed together," he said, "some chalk and vitriolic acid at the bottom of a large cylindrical glass; the strong effervescence produced an air or vapour, which, flowing out at the top of the glass, extinguished a candle that stood close to it; and a piece of burning paper immersed in it was put out as effectually as if it had been dipped in water; yet the smell of it was not disagreeable." This is the first note of knowledge respecting the physical action of fixed air—carbonic acid—on combustion, and marks out quite a new era in philosophical and experimental chemistry.

In 1755 the serious illness of Professor Plummer led to a contest for the chemical chair in the University of Edinburgh. Three candidates were brought forward—Dr. Francis Home, Dr. Joseph Black, and Dr. William Cullen. Home was supported by Lord Milton, an active lawyer and politician; Black had the support of the medical fraternity, who preferred to have a chemist rather than a practitioner of physic; Cullen had the support of Lord Kames, and the benefit of an immense reputation as a chemist. He also had the support of Black himself, who wrote to him two letters, informing him of Professor Plummer's condition, and stating that if he himself were elected to the chair he should refuse it. In the end Cullen, as was recorded in last ASCLEPIAD, was the successful candidate.

The election of Cullen to the chair in Edinburgh left the chemical chair in Glasgow vacant; and who so able to fill it as Black, already a distinguished alumnus, and formerly the Demonstrator of Chemistry in that place? He was elected, I believe, without opposition, and from the first was accepted as one of the greatest teachers of the science and art of chemistry that the world of science had ever seen. Speaking of this appointment Dr. Robison observes: "On whom could it be bestowed with so much propriety as on such an alumnus of the University,—on one who had distinguished himself both as a chemist and as an excellent reasoner; for I hesitate not to say that, excepting the Optics of Newton, there is not a finer model for philosophical investigation than the essay on magnesia and quicklime. Had this vacancy not happened it might probably have been the lot of Dr. Black to practise medicine in some provincial town of Britain or Ireland; and, thus occupied, the serious concern which he took in the cases under his treatment

would have absorbed his whole attention, and might, perhaps, have overpowered his feeble constitution. It was therefore equally fortunate for himself and for the public that a situation now presented itself which allowed him to dedicate his talents chiefly to the cultivation of chemistry, his favourite science."

The learned Professor is perhaps too enthusiastic, in this passage, in praise of his hero and his friend. A man can never be more nobly employed, whatever his talents may be, than in the practice of medicine anywhere; and such men as Black, so original and truthful, rarely fail to find some line of labour in medicine which proves of service to their fellow-men. At the same time we may be happy in recognising that Black found at once the field of work that was most suitable to his love and his genius; for when love and genius blend with daily labour happiness of the purest quality is laid, and life is turned into a dream of pleasure. Moreover, if Black had taken to the practical art of medicine in its laborious routine his life would probably have been a drudgery; since there is no profession, trade, or calling so hard to bear by those who are unfitted, by natural strength and will, to carry out its sacred duties. To such men—inadequate men for those duties—every act is a penance, and all life a march, bearing the cross of humanity.

Black found what he wanted; and in his entrance into the mere porch of the temple of fame made a distinguished mark. A copy of his inaugural essay was despatched, naturally enough, to the good old pater, still living at Bordeaux, and was transferred, also naturally enough, to the great Montesquieu, who, after reading it, expressed his views upon it in the warmest terms of praise. "Mr. Black, my very good friend, I rejoice

with you; your son will be the honour of your name and your family."

The professorship in Glasgow was not confined simply to chemistry; to it anatomy was added. With Black, however, anatomy was not a congenial study. He commenced to teach it with a conscientious resolution to carry out the duties faithfully, but soon made arrangements to exchange the anatomical chair with the Professor of the Institutes of Medicine. The exchange was excellent. His lectures on the Institutes fitted in splendidly with chemistry, and although he kept the two subjects apart, he drew a few rules of medical practice from physiology; and combined these with other rules deduced from successful practice alone, as distinct from physiology, which method he considered to be most conducive to a sound medical education. But it was in the study and the teaching of chemistry he most excelled, proceeding in the quietest and least obtrusive manner, with his mind always calm and free of ambition.

The most distinguished colleague in the University, at this moment, and whom the new professor could claim as a friend as well as colleague, was Adam Smith, the author of the "Wealth of Nations." Their friendship, Robison tells us, became more and more intimate and confidential through the whole of their lives. A certain simplicity of character, with an incorruptible integrity, which was acutely sensible to the smallest indelicacy or incorrectness, was instantly seen by each of these friends in the character of the other, and riveted the bond of their union. Dr. Smith used to say that no one had less nonsense in his head than Dr. Black; and he often acknowledged himself obliged to Black for setting him right in his judgment of character, confessing that he was apt to form his opinion too generally from a single feature.

“Indeed,” adds Robison, “were I to say what intellectual talent Dr. Black possessed in the most uncommon degree, I think I should say that it was his judgment of human character, and a talent which he had of expressing his opinion in a single short phrase, which fixed it in the mind never to be forgotten.”

At this period of his career Dr. Black carried out, in addition to his professorial duties, those also of a practitioner of physic. I observe that this part of his work has been denied altogether, or treated as if it were something of the mere amateur sort. Nothing can be further from truth. He practised actively and successfully; a fact testified by Robison from direct personal knowledge and observation. In 1763, according to this well-informed authority, Black was a favourite of the large and active city of Glasgow. His sweetness of manner, which the dullest eye might perceive to be free from all studious endeavours to please, and the evident concern which he took in the cases under his care, made him a welcome visitor in every family. His countenance, at that time of his life, as engaging as his manner, was attractive; so that in the peculiarity of the general popularity of his character “he was in particular a favourite with the ladies, who regarded themselves as honoured by his attentions; nor were these indiscriminately bestowed, but exclusively paid to those who evidenced a superiority in mental accomplishments, propriety of demeanour, and grace and elegance of manners.”

We have now followed Dr. Black into the first completed stage of his career. Henceforth we must look upon him as chemist above all else. He is still physician, and he may be held as such to the close of his life; but in that which he shall leave behind him, through which he shall

be known always, we must accept him as the original chemical explorer.

THE WORKS OF BLACK, AND THEIR RESULTS.

In a literary point of view Joseph Black was rarely great. He wanted the gift of the man of letters, and he felt it a burden to undertake and carry out any literary task. His labours were chronicled in his lectures, and we are mainly indebted to one of his pupils and colleagues for the preservation of his choicest efforts. As a matter of literature the quantity is, comparatively, small, although what it represents must have demanded arduous and long-continued study. All his work is comprised in two volumes quarto of about five hundred pages each, and is extremely irregular in respect to value. In dealing with subjects of a descriptive kind, in which the details of his own experimental researches are reported and explained, nothing in the whole course of scientific literature can be better; nothing more clear, sound, original, or far-seeing. We have before us a man who knows what he is about, and who tells what he knows without any desire to praise or appraise himself. We are led to feel the deepest interest in what he is explaining, and are anxious to forecast what he is about to disclose. Also, we are led to ask what will yet come out of that which has been revealed? There is opened, in reply, to us who live more than a century afterwards, a revelation rich in wonder and instruction; showing to us that work, which men like Black perform, never loses its freshness, but comes up, over and over again, as a fountain we ourselves are continuing to use and utilize. All this is most satisfactory in the descriptive parts of the work; but when we come to the more strictly philosophical portions, to argument apart from experiment, and to definitions which some would call abstract, we lose

our hold on his mind. He is not even delightfully obscure as many are. If he led us into a dream first, and then into a maze, and brought us out by waking us up in doubt and obscurity, we might be pleased and thoroughly grateful. We might possibly think that we had learned something, though we might not understand what it was or what it meant. Unfortunately, he leads us into the maze, taking care, all the while, to keep us awake, and to make us think over and analyse every word, whether we relish it or not. What is still more peculiar, he criticises some authors whose definitions seem to be as clear as crystal. In his first lecture on the definition of chemistry, he finds fault with Macquer's true, admirable, and simple definition that "chemistry is a science of which the object is to understand nature and the properties of all bodies by their analyses and their combinations." This, he says, is improper, "because in a great number of the operations which the chemist performs he neither divides compounded bodies into their principles, nor combines others which were separated before; nor has he either of these ends in view in any of his inquiries." In a similar strain he criticises other writers; and when he arrives at the point of defining for himself, he tells us that.—"Chemistry is the science or study of those effects and qualities of matter which are discovered by mixing bodies variously together, or applying them to one another, with a view to mixing, and by exposing them to different degrees of heat, alone or in mixture with one another, in order to enlarge our knowledge of nature, and to promote useful arts; or, in fewer words, that the chemist studies the effects produced by heat and by mixture in all bodies, or mixture of bodies, natural or artificial, and studies them with a view to the improvement of the arts and the knowledge of nature. This, in my opinion, takes in all that is proper to chemistry

and inseparable from it ; and, at the same time, leaves out everything that does not belong to the science."

We should not in the present day attach importance to the definition of chemistry here given, and if the reputation of Black rested on these passages it would be small indeed.

RESEARCHES ON HEAT.

The above definitions were made in the introduction to his collected works. There, in the first part, on the general doctrines of chemistry, he opens with an essay on the general effects of heat. He explains what he means by the word *heat*; next he defines the meaning of the term *cold*, and endeavours to ascertain the real difference between heat and cold. He mentions the attempts that have been made to discover the nature of heat, or to form an idea of what may be the immediate cause of it, and endeavours to describe the sensible effects produced by heat on the bodies to which it is communicated. Respecting the word *heat*, he uses it in one sense only ; namely, as expressing that state, condition, or quality of matter by which it excites in us the sensation of heat. Cold he considered to be a negation of heat ; but here he had to contend and overcome the theory, then extant, that cold was produced by frigorific particles, congealing particles which, by their actual presence, produced the effect. He succeeded in overthrowing this idea altogether, and in establishing the rule that coldness is only the absence or deficiency of heat. "It is the state most proper to common matter ; the state which common matter would assume were it left to itself, were it not affected by any external cause." "Heat," he assumes, "is something added to common matter, or some alteration of it from its most spontaneous state."

But what is this superadded something called heat ? To

answer this question he summons the great spirits of the past, beginning with Sir Francis Bacon, whose conclusion, drawn from the whole of his facts, is a very general one, namely, "that heat is motion." "Sir Francis Bacon," he says, "had many followers; but the great number of French and German philosophers, and Dr. Boerhaave, have supposed that the motion in which heat consists is not a tremor or vibration of the particles of the hot body itself, but of the particles of a subtile, highly elastic, and penetrating fluid matter which is contained in the pores of hot bodies, or is interspersed amongst their particles; a matter which they imagine to be diffused through the whole universe, pervading, with ease, the densest bodies; a matter which some suppose, when modified in different ways, produces light and the phenomena of electricity."

Another hypothesis which Dr. Black had to consider, and to which he attached very considerable respect, was that of Dr. Cleghorn, viz., that the assumed elastic matter pervading all space, and entering the closest parts of the densest substances, has not only the property of great elasticity, and of strong repellency of its own particles, but possesses a strong attraction for the particles of the other kinds of matter in nature which have in general more or less attraction for one another. This supposition would explain, he thinks, or is conformable with, some of the experiments of Dr. Franklin and Sir Benjamin Thompson.

It is most curious, as it is also most instructive, to read in this essay the very hypotheses which have, in our own day, been discussed and re-discussed so warmly. Heat as a mode of motion; heat as the vibration of a subtile medium, filling all space and permeating all bodies; heat as having powers of repulsion for itself, and of attraction for other particles, by which it both rends and combines matter (an hypothesis which Samuel Metcalfe so ably expounded),

are, after all, mere repetitions of the period when Black flourished. If he clung to any one theory or hypothesis, it was to the last named; but really he was not bound by any. Their first business, he taught his students, was to study the facts, and to attend to the manner in which heat enters the various bodies, or is communicated from the one to the other, with the effects which it produces. These effects, he said, are EXPANSION, FLUIDITY, VAPOUR OR INCANDESCENCE, AND INFLAMMATION OR COMBUSTION.

Following up his subject, under the heads named, Black is led, under the head of expansion, to treat on the thermometer. The invention of this instrument he credits to Sanctorius, who is distinguished by the discovery of what is called the "insensible perspiration;" but the editor of Black corrects this statement, and assigns the invention to a physician by the name of Drebel, living at Alkmaer, in Holland, in the beginning of the seventeenth century. A fine history of the thermometer brings to a conclusion this section of the subject and introduces the topic of the "distribution of heat," and of "the capacity of different bodies for heat." Here Black drew the inference that different bodies, though they be of the same size, or even of the same weight, when they are reduced to the same temperature or degree of heat, whatever that may be, may contain very different quantities of the matter of heat; which different quantities are necessary to bring them to the level or equilibrium with one another. This fact, he contends, is unfavourable to the opinion that heat is a tremulous or other motion of the particles of matter; because if such were true we must admit that the communication would be in conformity with our general experience of the communication of tremulous motion. The denser substances ought to be most powerful in communicating heat to others, or in exciting them. The

fact, however, in a great many examples, and yet not in all, is just the reverse. The opinion is, therefore, totally inconsistent with the phenomena, and he did not see how the objection could be evaded.

The "celerity of the communication of heat" formed a subject of much moment, and explained to the world many facts and inferences which were not well known previously. Here Black makes it clear that the heat of the sun is communicated, in the first place, to the earth, and is given out, from the earth, to the atmosphere; here he gives an estimate of the height of the atmosphere, namely, an approximate height of fifty miles, but with the upper strata very much attenuated. Here, also, he calculates that the diminution of heat is about one degree of Fahrenheit for every two hundred feet of elevation. The greatest difference of heat, therefore, balances two-fifths of the effect of compression, and cannot alone raise the air contiguous to the earth's surface to any considerable height. Were it not for the winds the region of perpetual snow, even under the equator, would not be a mile from the level of the sea. But, he adds, there is a gradual and indefinable transition from the warmth of the lower air to the coldness of the higher, and in the higher regions the same gradual diminution of heat and compression.

CAUSE OF FLUIDITY.

Touching "fluidity," Black taught that we have the strongest reasons for considering heat as the efficient cause of fluidity, in all bodies in which this quality is found. He made this statement, as he believed, a matter of clearest demonstration by his experiment of dissolving ice in warm water, and illustrating from the experiment what he denominated the absorption of heat and the rendering of heat latent. His "summary" on this

matter is submitted in the following words, rarely, if ever, quoted in full, but conveying his hypothesis in the most striking and intelligible terms:—

“I consider fluidity as depending immediately and inseparably on a certain quantity of the matter of heat, which is combined with the fluid body, in a particular manner, so as not to be communicable to a thermometer or to other bodies, but capable of being extricated again by other methods, and of reassuming the form of movable or communicable heat.” In this way he accounted for the specific capacities of bodies for heat. “If they act on each other, so as to produce solution, or even softening of one of the bodies, heat will be absorbed; or if the mere heat of the mixture melts or softens them.” He applies this same rule to firm but malleable substances like iron and copper. If iron be beaten with a hammer its latent, or banked-up, heat will be rendered current, movable, communicable, sensible heat, because the heat, so to express it, has been beaten out of the iron. The heat was not communicated to the iron by the hammer, as by a mode of motion, according to the Baconian idea; for if it were so, when the iron had cooled it could be brought back to the same glow at the same part, and would anneal by a repetition of the blows of the hammer. But now, to repeat the effect of producing heat from motion, the iron must be replaced in the furnace, in order to absorb and render heat, again, latent; then responding to the blow the metal will become quickened into the bright glow of sensible heat, movable and communicable.

LATENT HEAT.

From the study of heat as a cause of fluidity Black proceeded to “vaporization” and the heat concealed in the vapour of water. He conjectured that during the

boiling of water heat is absorbed by the water, and enters into the composition of the vapour produced from it, in the same manner as it is absorbed by ice in melting, and enters into the composition of the produced water. And, as the ostensible effect of the heat, in this last case, consists, not in warming the surrounding bodies, but in rendering the ice fluid, so, in the case of boiling, the heat absorbed does not warm surrounding bodies, but converts the water into vapour. In both cases, considered as a cause of warmth, we do not perceive its presence; it is concealed, or latent, and, "I gave it," says he, "the name of *Latent Heat*."

A little further on in the same discourse he repeats his proposition in another and more striking form, putting the words in italics: "*When a fluid body is raised to its boiling temperature, by the continual and copious application of heat, its particles suddenly attract to themselves a great quantity of heat, and, by this combination, their mutual relation is so changed, that they no longer attract each other, gathering into drops and forming a liquid, but avoid each other, separating to at least ten times their former distance (for a cubic inch of water forms much more than a thousand cubic inches of vapour), and would separate much further were they not compressed by the weight of the atmosphere; and, in short, they now compose a fluid, elastic and expansive, like air.*"

EVAPORATION.

Studies of this nature caused Black to enter into many practical enquiries belonging to his time. He had as a pupil, friend, and fellow-worker the illustrious James Watt; and almost of necessity the steam engine, which he calls "the masterpiece of human skill," came under his observation. He describes the first steps which

led to this invention, from the early labours of Papin and the Marquis of Worcester to those of Newcomen and Watt. Into this description we need not follow him, interesting as it is, but may pass to his studies on what he calls spontaneous evaporation of water from expanses of surfaces. He now brings us to a physiological effect, up to his time unrecognised, on the natural cooling of the human body. There is, he says, in the body itself a continual laboratory or manufacture of heat; and, were the surrounding air of such a temperature as not to carry it off, heat would soon accumulate so as to destroy life. The excessive perspiration, supplied by diluting draughts, performs the same office as the cold air outside the tropics in guarding us from this fatal accumulation. "It is not unlikely that the constitution of the vessels of the lungs and the pores of the skin, which unfits them for bringing forward the lymph in sufficient quantity for carrying off, by evaporation, the heat generated by the vital functions, is the immediate cause of the heat in ordinary fevers." In illustration he explains to us what heats may be carried off in this way. Dr. Fordyce stayed, without great inconvenience, in a room heated to 260° Fahr. The lock of the door, his watch and keys lying on the table, could not be touched without burning his hand, and an egg became hard. His pulse was at 139; yet a thermometer held in his mouth was only 2° or 3° hotter than ordinarily. He perspired profusely.

The close of the first section of the work of Dr. Black is on the important services rendered by heat in nature. It is a wonderful peroration. In it fact combines with a true poetry in prose, which shows the brightness and genius of the man at the strongest period of his career. The part assigned to water in the creative scheme was never more splendidly sung; the description is as clear

as the purest stream that water ever made; the accent as musical as the sweetest rill. The author literally warms to his theme. "Were the heat which at present cherishes and enlivens this globe allowed to increase beyond the bounds at present prescribed for it, besides the destruction of all animal and vegetable life, which would be the immediate and inevitable consequence, the water would lose its present form, and assume that of an elastic vapour, like air; the solid parts of the globe would be melted and confounded together, or mixed with air in smoke and vapour; and nature would return to her original chaos."

CHEMICAL ATTRACTION.

After treating on the subject of heat Dr. Black proceeds to that of chemical attraction. He gives to Sir Isaac Newton the credit of the suggestion of this kind of attraction in minute particles of matter, and defends the use of the term "attraction," as employed by Newton, against that of "affinity" applied by his successors. He seized the idea of the minuteness of these attracting particles by estimating, from experiment, that a grain of silver could be divided into eighteen millions of sensible parts. He observed that the energy of chemical attraction is so limited, round the particles of bodies, that it is not perceptible, and that if we trusted to our senses no action would seem to take place until the particles came into the closest contact.

A section on chemical apparatus is rich in historical facts bearing on the appliances possessed by the most accomplished chemists a little over a century ago. Everything, as he describes it, is so simple, and the whole is so limited, in comparison with what we have now at command, that the chapter reads almost like a

child's story. This chapter is succeeded by one on salts and crystallization, in which much curious matter is discussed calling for renewed enquiry, especially in relation to the action of light on crystallization, which Black believed to be negative, but which the editor of his works, following Chaptal, looked upon as proved. Equally curious are his accounts of earths, alkalies, acids, and salts, with which the chemist manipulates.

It is necessary for me to pass over these subjects of enquiry in order to arrive at that part of the labours of our great experimentalist which relate to his extraordinary suggestions and observations on what was called, by him, "fixed air." Upon his researches at this stage a new era in vital chemistry becomes developed, on which we must for a few minutes bestow a close attention.

RESEARCHES OF FIXED AIR.

The experiments with which Black commenced his enquiries on the subject of fixed air were made on lime; to some extent they had commenced with magnesia as well as lime, but lime was the material chiefly used. He began his new series of research with lime water. He made a very strong lime water, and placed four ounces of it under the air pump, along with four ounces of common water in another vessel of the same size. The air was taken out of the receiver, and, while this was doing, air bubbles formed and rose in both phials, in equal quantities, and in the same manner, as far as he could judge, while the lime water continued perfectly transparent. From this he concluded that the air escaping from the lime water had been combined with the water, and not, as had been previously taught, with the lime; he also concluded that the air which water commonly holds in solution is of a different nature from

that which is attracted by lime and alkali. For had it been the same, and combined with the lime, as fixed air is, the removal of the atmospherical pressure would not have been sufficient for occasioning its separation. Quicklime, therefore, does not attract the air that is usually contained in common water, nor does it attract the whole of the mass of atmospherical air. It attracts only a particular kind of aerial fluid, which is mixed with the air of the atmosphere, in the way of common diffusion, and in a small quantity only. It is mixed as spirits are in water, or one metal with another, without any change of properties; and alkaline substances take it out, as aquafortis takes out the silver which alloys a piece of gold.

Here, says Black, "a new and perhaps boundless field opened before me;" and then he proceeds to teach what had been revealed concerning fixed air. Van Helmont had given to an elastic body the name of "*gas silvestre*," and had supposed that it was the chief part of the gas given off from fluids in a state of fermentation. The same authority had conjectured that this same gas is given off from substances that are undergoing combustion; a random conjecture, as Black thinks, because he (Helmont) did not know a fact understood by the chemists of the later day, that the gas extinguishes flame. Dr. Macbride of Dublin had obtained some of the same air from fermenting vegetables, and from animal substances in a putrefying condition. The Hon. Henry Cavendish had separated, from alkalies and earths, by acids, a permanent elastic fluid, some of which he kept for twelve months in a vessel inverted into mercury, without any diminution of its elasticity. Cavendish also discovered that this gas was absorbable by water and some other fluids, and had calculated the quantities of it contained

in marble and other bodies; he obtained the precise density of it, and showed the density to be greater than that of common air in the proportion of 157 to 100; in consequence of which it lies at the bottom of a vessel, and may be poured out like water, extinguishing a candle as water would do. After Mr. Cavendish, Dr. Brownrigg came on the field, and indicated that ordinary water, charged with this gas or air, acquired all the qualities of some "spa waters," known as "acidulous mineral waters," brisk in property, and possessing the power of changing the infusion of litmus to a red colour.

The most remarkable series of facts to which Black refers were, however, those elicited by Lavoisier, who, having burned charcoal in pure oxygen, collected the aerial product, and found that the product was the result of a combination of the oxygen with the carbon; which experiment and induction were not strictly original, since they had both been anticipated by Hooke, who had explained long previously that the nitro-aerial spirit of the air, described by Mayow, combines in the combustion of bodies, like wood or charcoal, and that the product is a volatile compound which diffuses into the air.

In books on chemistry it is often stated that the discovery of this product from combustion of carbon was first made by Black, and that to him should be given the whole credit of the discovery of the production of fixed air and the evolution of it by the breath in expiration. In point of fact it is not quite so. Lavoisier demonstrated the production of the gas by the combustion of carbon in oxygen, and finding the gas to be acid gave to it the name by which it is still known, carbonic acid; and, Black himself records, that the production of the gas as a result of animal combustion had been proved "by the experiments of Scheele,

Lavoisier, Goodwin, and Menzies." He (Black) demonstrated the same truth; and although he did not first disclose it, he made the truth so clear, and removed so sharply the many obscurities by which it had been surrounded, that he seems to be the prime and natural authority. In some senses, too, we may look upon him in that light; for he it was who made proof so easy that all could repeat it. For instance, he first showed the lime test for the gas. He found that by blowing it through lime water the lime was precipitated. It was he who first collected the gas from the surface of wort in a brewery, and on shaking up the gas, so collected, with lime water found again the lime precipitated. He performed another experiment. He unfixed the nozzle of a pair of bellows, put into the nozzle a bit of charcoal, just red hot, and then quickly replacing the nozzle, plunged the pipe into the bottom of a phial, and forced the air very slowly through the charcoal, so as to maintain its combustion, but not to produce a heat too suddenly for the phial to bear. When the air of the phial was completely vitiated he poured lime-water into it, and had the pleasure of seeing the water become milky in a moment. More curious still, he observed and recorded a fact, thought to belong to our own time, that carbonic acid may make the lime in water either soluble or insoluble, according to the proportion of it that may be present. Thus he observes: "It may appear surprising to you that the same substances which, added to lime-water, precipitate the lime by making it insoluble, should also be the cause of its re-dissolution when added in larger quantity. But the fact is certain, and it is not singular. For example, most of the compound salts can be made more soluble in water than they are in their perfect or neutral state by adding to them a superfluous quantity of acid."

It is, perhaps, fair also to Dr. Black to record that he first investigated the toxic action of fixed air or carbonic acid. He, too, was one of the earliest physicians to recommend its use in the treatment of disease. He commends it as a grateful substance added to water, and he speaks of its value as an outward application to ulcerated surfaces of the skin. He says that, when inhaled with five or six times its volume of common air, it is a good remedy, and has been found useful in the treatment of consumption and ulcer of the lungs. It is worthy of notice that Black thought the gas was less dangerous to breathe when it was taken in by the mouth than when absorbed by the nostrils. He observed this fact, particularly, in watching its action on sparrows.

I have observed that Black gave credit to the experiments of many of his predecessors and cotemporaries on the subject of carbonic acid, or, as he named it, fixed air. At the same time he claimed that he made its nature and properties clearly demonstrable in the year 1756; and it is incontestable that in this he was only declaring his own just right. Before his demonstrations all was conjecture, or part conjecture and part demonstration; he brought everything into demonstration. Some changed the name he had given of "fixed air" to "fixable air," after Mr. Cavendish; others called it "acidulous gas." Henry of Manchester and Professor Bergman called it "aerial acid;" Fourcroy called it "*acid crayeux*," acid of chalk; Lavoisier called it "carbonic acid," and this last name it has retained to our time; but Black isolated it completely when he published its properties as "*fixed air*."

The remaining portions of the works of Dr. Black are full of understanding, suggestion, and learning; yet they are rather of a professorial than of an original character. They are the labours of a teacher of chemistry anxious to

lay before his students all that could be usefully collected in the then field of chemical enquiry—a new field, and most laborious. Sometimes he was led from the chemical side of his work to the medical. Thus, in treating of iron, he refers to iron as a medicine, and relates the not very accurate knowledge of his time on that topic. He takes pains to illustrate the various instruments used in chemistry, and to classify the different varieties of chemical substances; in which effort he is far in advance of his immediate compeers. He is conspicuously fair to his fellow-workers in his descriptions of their services to science, and his account of the researches of Priestley and Cavendish excel in candour. In openness of spirit no author is more commendable. He enters into the thoughts and deeds of the actors as if he were one of them, and he delineates the course of discovery with a degree of conciseness that makes the reader grasp even grand progresses with an ease as refreshing as it is satisfying. The history of the discovery of “faul air,” afterwards called “nitrogen,” and still later “azote,” is a case in point.

THE CLOSE OF THE LIFE.

In the above notices of the works of our great scholar I have been led away from his career and his personal life. To some particulars bearing on these subjects it is now necessary to return. Fortunately we can return with advantage, for our authority, Professor Robison, was on the spot where Black worked, and was his familiar friend, pupil, and fellow-student. From Robison's memoirs we see Black, during his Glasgow life in the old University there, simple as a child, communing with nature, and yet fixing his gaze on secrets of nature which the highest reason might be proud to reveal. He had a pupil, at this time, working away in a small workshop opposite the

University, where, in my time, we students bought our books of worthy Mr. Haddon, a bookseller, who never let any freshman leave his door without informing him that he stood in the birthplace of the locomotive steam engine, in the very shop where James Watt constructed his first model of that marvellous machine. Robison, on this spot, saw with his own eyes what his pen has written for us in a picture as living as when it left his living hand. Watt fitted up, from his shop, the instruments that then were used in the McFarlane Observatory of the University. He chanced to have in his hands for repairs a model of Newcomen's steam engine, belonging to the Natural Philosophy Class, and was delighted with the opportunity which this small machine gave him for trying experiments connected with the theory of ebullition, which he had learned from Dr. Black; "and he did not stop until he had made his steam engine more like the most docile of animals than a frame of lifeless matter." Watt, attached to Black by every tie of respect, esteem, and affection, supplied him with proofs and illustrations in abundance of all points on which the Professor wanted information. These were recited to the class—the fortunate class that saw the mighty engine of mankind put together in its infancy, and nursed into growth by the genius who understood its powers as well as its nature. One thing gave the master of this genius much satisfaction. Watt, who had obtained His Majesty's patent for the improvements he had made in the steam engine by his judicious application of Dr. Black's instructions, was now on the straight road to riches and fame; and Black would scarcely have been more gratified had those advantages accrued to himself. This is Robison's testimony, the testimony of one who, as Professor of Natural Philosophy, was directly concerned in these splendid triumphs of practical science.

Dr. Black held the Chair of Chemistry in the University of Glasgow for ten years, namely from 1756 to 1766—"much respected," said Robison, "as an eminent professor; much employed as an able and most attentive physician; much beloved as an amiable and accomplished gentleman; and happy in the enjoyment of a small but select body of friends." His reputation as a chemical philosopher kept increasing. Pupils came from all parts, and carried to all parts his peculiar doctrines on fixed air and latent heat. But progress would have been slow except for a wider sphere which was opened to him from the resignation of the Chair of Chemistry in the University of Edinburgh by Cullen, and his appointment to the vacant seat. His election to the chemical chair met with general consent, and his fame as a teacher and man of science was vastly extended. He became now more of the Professor and less of the original inquirer. He became one of the most favourite of lecturers, attracting to his lecture-room *dilettanti* as well as serious students. Under these conditions teaching became the first object of his life; but, unhappily for the world, his health began so seriously to fail him that every kind of added exertion produced a prostration which crippled him sorely. The slightest cold, Robison states, brought on spitting of blood, from which nothing restored him except repose and relaxation of thought, with gentle exercise. His natural bent was to follow up the pneumatic chemistry which Priestley had set forth, and to fight out the new problems that were springing up, with the most active of his compeers. But it was not to be. The spirit was willing; the flesh was weak. He followed rather than led; yet sometimes his original energy still exerted itself, as a correspondence with Lavoisier shows. Lavoisier had triumphed over the German school of chemists, which then, as now, claimed

to lead the chemical world. Lavoisier had triumphed over the great German Stahl himself, and Madame Lavoisier (afterwards Countess Romford), dressed in the habit of a priestess, had burned on an altar Stahl's *Chemiæ Dogmaticæ* to the sounds of solemn music. The scene was dramatically absurd; still Black supported Lavoisier until that erratic star provoked him by ignoring him in his published writings, while flattering him "fulsomely" in his private correspondence. In this transaction Black stands forth with a simple grandeur which the unhappy death of Lavoisier at the guillotine does not overshadow.

In the community of Edinburgh Black was simply beloved. My late dear friend, Professor Macdonald, of St. Andrew's, knew intimately some who remembered Black personally, and who spoke of him as of one belonging to a distinct sphere of intelligence and goodness. Robison says of him that in early years he was comely, and in later days pleasing. His manner was easy, unaffected, graceful. His voice was gentle and musical, and his accomplishments in music and drawing above the average. "Love of propriety was the leading sentiment of his mind;" to which Dr. Ferguson adds that "he loved to promote social conversation by every cheerful thought that occurred. Many years member of a society of noblemen and gentlemen of first rank—of judges, lawyers, and professed men of letters—he kept his place with the most easy propriety, having knowledge sufficient for giving an interest in the conversation of each, and for taking a respectable share in it, without exhibiting any peculiarities arising from his more accustomed habits of thought. This society, and another small evening party, or club of gentlemen, more professedly scholars, were the only public companies which his delicate health permitted him to frequent."

In his latter years Dr. Black continued in the same frame

of contentment as in his younger life. Nothing seemed to disturb his natural serenity. When he was leaving Glasgow he lost three-fourths of his substance by the failure of a house in which he had invested his savings, and at another period he ran a most serious monetary risk in serving a friend; but he never complained or repined. As he declined with the century he had done so much to immortalize, he rested greatly on the judgment and care of his friend Dr. James Hutton, who "shared all his thoughts." He lived the most regular and almost abstemious life, and, anxious only to be saved a lingering sickness as prelude to death, was ready for the final change. His wish was granted. On November 26th, 1799, whilst seated at table with his usual fare—some bread, a few prunes, and a measured quantity of milk diluted with water—he sank. He had the cup in his hand when he was seized, and set it down on his knees, which were joined together, holding the cup steadily in his hand, like one perfectly at ease; and in this position he expired without spilling a drop and without a writhe on his countenance, as if, says the narrator of the event, "an experiment had been required to show to his friends the facility with which he departed."

In Monro Primus energy and business shone; in Cullen industry; in Black genius, insight of the highest order. Black was one who made history. Nowadays some men, recalling Baconian hypotheses, are holding up once more Stahl and his phlogiston. But they leave Black alone in his glory.

REPORTS, PHYSIOLOGICAL AND THERAPEUTICAL, MADE TO THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FROM 1863 TO 1872.

I. ON THE PHYSIOLOGICAL PROPERTIES OF NITRITE OF AMYL AND OF NITRATE OF AMYL.

FROM REPORT III.—READ IN DEPARTMENT D., BIOLOGY, ON MONDAY, AUGUST 7th, 1871, AT EDINBURGH,

[PROFESSOR ALLEN THOMSON, M.D., F.R.S., IN THE CHAIR.]

Continued from page 189 of ASCLEPIAD, No. 26, 2nd Quarter, 1890.

IN one of my early papers on nitrite of amyl I pointed out that the effects observed were clearly due to an induced paralysis of the vascular system, of the terminal part of that system, and that the heart passed into vehement motion, not, as I at first had thought, because it was excited by the agent, but because, the resistance to its action being removed, it ran down like a clock in which the resistance to the spring is broken by the removing of the pendulum or the pallets. I further explained that the seeming over-action produced by the nitrite was in truth no evidence of power or tension of muscle, but that in truth, under the influence of the nitrite, the muscular system is brought into extreme relaxation, so that the substance might be used as a remedy for the relief even of tetanic spasm. These views have been sustained by later observation. It remained, however, still to discover how far the relaxation of vessels from nitrite of amyl extended to the functions

of special organs of the body ; and during the present year I have followed up this line of research in respect to the changes producible by it in the pulmonary organs, the lungs. The study has been most fruitful, and will, I think, as it is followed up, open quite a new field of accurate and sound observation as to the mode in which many diseases of the lungs take their origin.

In previous reports I have entered at length into details of action, of which the following is a summary.

The nitrite, though insoluble in water, will enter the body and produce its specific action by any channel of the body, by the cellular tissue, stomach, blood. It produces general muscular paralysis, affecting directly or indirectly all the motor centres.

It exerts no primary action on the sensory centres, and therefore does not produce anæsthesia.

Its paralyzing action seems first to be directed to the organic nervous centres, by which the vascular tension is reduced. It acts, in fact, after the manner of an emotional shock, leading quickly to paralysis of the minute vessels.

It prevents oxidation by its presence, and possesses distinct antiseptic powers. It produces a peculiar tarry condition of the blood, but does not materially impede coagulation.

It neutralizes the tetanic action of strychnia, and removes tetanic spasm.

On reviewing these inferences of former years, as thus detailed, I see no occasion to change one of them ; indeed I believe they have, on the whole, all been confirmed by other observers. The admirable experiments of Dr. Brunton on the action of the nitrite on vascular tension call for special recognition. There is, however, an observation in my report of 1864 I would like to correct.

Speaking at that time of the action of the nitrite on the muscles, I remarked that it first excites the muscular system, and then paralyzes it. I am in doubt now whether the muscular excitement of which I spoke in 1864 is a true excitement due to the influence excited by the agent on the motor centres. I think, from my own sensations, it is rather due to an indirect or mental impression, that it indicates a vehement desire to escape from the influence of the agent, like the excitement of fear or frenzy.

SYNTHESIS OF PULMONARY CONGESTION.

Let me from these points turn to the observations of the past year. I observed long ago, in making dissections of the bodies of animals that had died from amyl nitrite, that the condition of the lungs varied much, that sometimes the lungs were of milky whiteness, sometimes of leaden hue, and again of deep dark red hue. It occurred to me at last that these differences were not accidental, but that they depended upon the mode in which the nitrite destroyed the life of the animal. Thereupon I made direct inquiry into this subject, and was led to discover that I could, practically, modify the circulation of the blood passing over the lungs from the right to the left heart, as I pleased; in other words, I learned that the vessels of the lungs are influenced by the nitrite in the same manner as the vessels of the skin.

The observation thus stated led me naturally a step further. I inquired as to results of different temporary lesions that might be inflicted on the pulmonary organs by the nitrite, and what extremity of lesion could be recovered from under conditions favourable to recovery. I commenced this research in February last (1872), and have carried it on without intermission from that time: the results of the labour have been most instructive.

SUMMARY OF SYNTHESSES.

There are four distinct conditions of lung producible by nitrite of amyl:—

1. If the animal be destroyed by an overwhelming dose of the agent, so that it dies instantly, as it might die from syncope, the lungs are left absolutely bloodless and of pure whiteness. The right side of the heart is in this case paralyzed; but exposed to the air immediately after death it often recovers its power of contraction. In this instance, the death is really by syncope; the nervous paralysis is extended immediately to the heart, probably from paralysis of the sympathetic supply, and the right ventricle failing to pour out its blood to the lung, the death is so instantaneous there is no time for the production of any organic change.

2. If the death be comparatively slow, if it be preceded by a short interval of muscular prostration, and if it occur from paralysis of the muscles of respiration, then the lungs are left charged with dark tarry blood, but they contain air and are free of congestion. Here the lungs and heart have failed together, and the balance of the pulmonary circulation has been fairly maintained.

3. If the effect of the nitrite be more definitely prolonged there is produced intense general congestion of the pulmonary vascular system, a congestion so intense that the lungs, full of blood, dark and heavy, will not float in water. The cause of death in this instance is progressive neural paralysis of the pulmonary vessels; the equivalent of congestion from long exposure to extreme cold.

4. The above may all be considered as acute changes in the pulmonary structures, and the two first-named changes are immediately fatal. The last need not be; as it occurs from prolonged and sustained action of the nitrite, in

quantities insufficient to kill directly, the effects of sustained congestion may be traced out from day to day for many weeks.

CHRONIC CHANGES. PULMONARY HÆMORRHAGE.

To be accurate in the observations made on this subject, I constructed a glass house or chamber of a capacity of three cubic feet, and so ventilated it that the air could be kept charged with the vapour of the nitrite. In this chamber rabbits and guinea-pigs were housed. They were carefully fed, supplied with abundance of air, and well protected from cold. The introduction of the vapour was so moderated that the same quantity was made to undergo diffusion each day.

The first fact that became well established was, that cold and a low barometric pressure greatly assisted the action of the vapour, and frequently led to sudden death from congestion of lung and accumulation of fluid in the bronchial tubes.

A second fact, of singular interest, was the degree to which recovery from extreme congestion of lung would take place, on simply withdrawing the animal from the influence of the agent. When the lungs were so obstructed that what is called *râle* from accumulation of mucous fluid in the bronchial surface was most marked, there was invariably a rapid recovery on removal to fresh and warm air.

A third fact relating to the lesions induced, beyond mere congestive lesions, is also of deep interest. The lesions were primarily all of one kind; they were hæmorrhagic, and consisted of red spots and patches, in which blood was effused and coagulated in the connective tissue. The position of the hæmorrhage was singular. In three cases it was only in the extreme point of the apices of the lungs;

in four other cases lower portions of lung were involved, but in these the apices were the seats also of hæmorrhagic disease. It would appear, in fact, as if these points of lung were least resistant to the force of the circulation.

In the animals observed these distinct hæmorrhagic changes were usually fatal, so that the further result of the local neural paralysis could not be carried out as could be wished. In two cases, nevertheless, we had other results.

In one instance there was clearly an œdema of the lung structure. In another the pleural membrane was raised in four or five granulated points, round each of which there was effused blood, and Dr. Sedgwick, who took the lungs of this animal for careful microscopic inspection, reported to me that there were plastic exudations in various parts of both lungs, and that the granulations of which I have spoken consisted of effused plasma beneath the pleura.

I am well content to leave these observations as I have written them above, with but two observations more. It has been suggested by an accomplished English physician, Dr. Eade, of Norwich, that pulmonary consumption may be primarily due to pulmonary vascular paralysis. My experiments do not enable me at this stage to endorse Dr. Eade's hypothesis as to the primary origin of consumption, but certainly they indicate to what extent nervous deficiency will go in favouring the hæmorrhages, congestions, and exudations which attend tubercular disease.

CURATIVE ACTION OF THE NITRITE ATMOSPHERE.

The concluding observation this year with nitrite of amyl relates the singular fact that the nitrite atmosphere, when it is not too much charged with the vapour, exerts a certain curative effect. Three rabbits were brought to me with a skin disease resembling lepra in man. They were emaciated and feeble, the fur on the

back along the whole length of the spine had been cast off, and the skin was covered over this part with white ashy scales.

Placing these animals in an atmosphere of nitrite of amyl, I noticed that, as the vapour took effect, the scaly white skin on the back became red and flushed. In a day or two the scales disappeared, the fur began to extend, and the general health to improve. In a month all the animals had entirely recovered.

There are many local conditions of disease in man and other animals in which the essence of cure lies in re-establishing a good capillary circulation. It may be, therefore, that by administering the nitrite of amyl, or the other organic nitrites, *secundum artem*, we may make them very useful in the cure of disease, and thereby add another progress to physiological as distinguished from empirical medicine.

NITRATE OF AMYL.

In one of my previous Reports I touched, incidentally, on certain of the *nitrates* of the organic series of compounds. These substances differ from the nitrites simply, in that they contain an additional equivalent of oxygen. It is a very interesting study to follow the difference of physiological action upon so simple a change of chemical constitution; and this year I studied this difference from nitrate of amyl.

Nitrate of amyl, $C_5H_{11}NO_3$, is a pale amber-coloured fluid, of not very agreeable odour. It has a specific gravity of 0.992, a boiling-point of $138^\circ C.$ (280° Fahr.), and a vapour-density of 66°. It is made by acting with strong nitric acid, 30 grms., on urea nitrate, 10 grms., adding afterwards 40 grms. of pure amylic alcohol. I am much indebted to Mr. Ernest Chapman for a

specimen of nitrate of amyl, freed as far as possible from nitrate of butyl, from all trace of which it can with difficulty be separated.

In the nitrate of amyl we have a substance differing chemically from nitrite of amyl in holding an additional equivalent of oxygen, and differing physically in that it is heavier and of higher boiling-point. It enters the body, readily, by all channels, and in its general effects it agrees with the nitrite, except that a longer time is required for the development of symptoms from it, and a longer time is demanded for the process of recovery from its influence. The quantity necessary to produce decisive results is the same as with the nitrite; but the nitrate is not so pleasant a substance to administer, and when administered by inhalation is not so conveniently applied.

RETROSPECTIVE NOTE—1890—ON AMYL NITRITE.

The above sentences occur in my final Report to the British Association on amyl nitrite. How, in the twenty-seven years which have followed the introduction of this chemical substance into medical practice, it has been employed as an antispasmodic, and what services it has rendered I need not tell. There is no form of acute spasmodic affection for which it has not been employed with some benefit; and it is most satisfactory to recall the fact that it was my good fortune to discover and establish its rôle in practical therapeutics, and to define its *modus medendi*. Still further properties of it remain to be applied, especially its curative action, by repeated and continued inhalation, in depressed states of the circulation and the nutrition. This also will become common knowledge and practice in due time.

THE EXECUTION BY ELECTRICITY.

APITAL murder was never more thoroughly discredited than in the late method of killing the unhappy culprit Kemmler by electrical discharge. What occurred was really worse than was prognosticated. The shocks administered were intense surface shocks, attended with extreme local action, but not affecting directly or immediately the respiratory centres. The idea seems to have been that an electric shock passed through the head would kill like the blow of a pole-axe; an idea which a perusal of one of Benjamin Franklin's experiments, in last century, ought to have rectified. The doubts which have been expressed about the strength of the current and the faults of the apparatus are out of the record. The mistakes were physiological, not electrical. If the blood, which is the prime conductor of electrical vibration, had been made the conductor, a current of half the strength used would have sufficed. If the electrodes had been applied to the arms of the man, one electrode to each arm, with saline and damp sponges, the death would have been more determinate, and far more rapid. The man was really killed by a clumsy stun, for which a dexterous blow from a pole-axe would have been an expeditious substitute. He died, as I described in commenting on the action of lightning stroke in 1868, by a kind of gaseous apoplexy from the tension of the

gases of the blood, not by the direct action of the electricity on either the structure of the nervous system or on the muscles. At the same time, whilst condemning, in the strongest terms, the degradation and the rank immorality which has been committed in the name of science, it is fair and proper to state that the man Kemmler suffered nothing. The electrical stun is a stun too quickly applied to be painful. I can cite two evidences of this fact. In the course of one of my experimental researches an assistant, from carelessness on his own part, was knocked over by an electrical discharge, and was, for a few moments, rendered quite insensible. He was lifted into a chair, and soon regained his consciousness. The strange part of the matter, to those who are not acquainted with the experiments of last century, is, that he not only did not feel pain, but positively did not know that the accident had ever occurred, and thought that a joke was being played on him when he was told the facts that had been observed. This is the first bit of satisfactory evidence. The second is that during a stun of this character the body lies so insensible that a surgical operation can be carried out without the infliction of any sense of pain. Thus, in one instance in which a large dog was stricken insensible by a single discharge, I performed a surgical operation—the removal of a tumour from the animal—without causing the slightest indication of suffering. There have, likewise, been instances in which injuries inflicted, after lightning shock, on the human subject, have not been recognized by the subjects of them until recovery from the shock has taken place. We may be certain, therefore, that the unhappy Kemmler, who, whatever his previous faults may have been, comes out as the bravest figure of the tragical scene, was not really subjected to agony. To him the godlike power was more merciful than the powers who

evoked it; as if, in the phraseology of a past day, "the heavenly might came to his aid, and seeing his fortitude, unchanged by the many deaths to which the savagery of his age had subjected him, gave him peace both of mind and body."

An ethical question is here opened. Ought the members of the great profession of medicine, who live to prevent pain, disease, death,—ought they to lend themselves, under any circumstances, to the loathsome act of playing the part of public executioner? I, for one, answer emphatically, *no*. On the contrary, we ought one and all to strike at these barbarities, and show the world that whatever insanity it may perform, we are no parties to it. It has been often and influentially suggested that a lethal chamber, such as I have constructed for the euthanasia of the lower creation, should be applied to the execution of criminals. I protest against the suggestion as subversive of the entire object of the invention; and I protest most strongly against any method of execution like electricity, or other, that shall call for the necessary aid of any man of science to carry it out. It is high time for our gaol surgeons to ask themselves whether they ought any longer to condescend to perform the miserable so-called duty of professionally witnessing capital murder. But, to take the leading part in it, *never!*

COTEMPORARY PRACTICE AND LITERATURE.

*“Every physician will and ought to make observations from his own experience ;
but he will be able to make a better judgment and juster observations by com-
paring what he reads and what he sees together.”*

“HYGIENE AND PUBLIC HEALTH.”

[BY LOUIS E. PARKES, M.D.]

HE name of Parkes is intimately connected with the history of the sanitary reformation in this country, through the labours of the late Professor Parkes of Netley ; and, in this little and convenient volume the relative of that eminent scholar preserves the name with all industry and honour. That the first edition of this volume, (H. K. Lewis) should have been sold out in six months is a record at which no one will be surprised who will give to the book careful study. The work is constructed, largely, for a specific purpose, namely, to serve as a manual for those who are preparing for the Public Health Diplomas of the Universities and Medical Corporations. It is, therefore, what is called “precise” in its lessons, and is of necessity clear of all that pertains to be speculative, except in matters which are by orthodox belief and acceptance admitted, for the time, as if they were true. It often happens, in the course of scientific history, that views and beliefs, in themselves the most wantonly speculative, and destined ultimately to pass away like an idle wind which no one regards, have to be recognised as true and demonstrative in their day

and generation, and to be impressed in text-books with all the solemnity of articles of faith. Hence, although in this book the germ-theory, or, more correctly speaking, hypothesis of infectious disease, is accepted, in accordance with the widespread faith in it, the statements upon which it rests are selected with as much attention and circumspection as can be applied to ephemeral and half-explanatory expositions of phenomena still in obscurity and waiting for elucidation based on sound premises. At the same time, the author is most anxious, and properly anxious, to guard himself from too sworn an advocacy, for he tells us that "it is more than probable that the future will bring forth modifications and alterations in principle and detail which will deprive the views at present entertained of much of that simplicity and easy intelligibility which is one of the chief recommendations to popular favour." The advice is shrewd, comprehensive, and far-seeing; and, acting upon it, the wise student will, it is to be hoped, not only read, mark, learn, but also inwardly digest it. In later editions the author may, with advantage, enlarge and improve the chapters on feeding and clothing; but, taking it all in all, he has used the space at his command ably, and has produced a manual full as an egg of useful information.

*"THROUGH THE IVORY GATE. STUDIES IN PSYCHOLOGY
AND HISTORY."*

[BY WILLIAM W. IRELAND, M.D.]



UNDER the singular title, "Through the Ivory Gate," the learned author continues his work entitled, "The Blot on the Brain," in which the characters of Mohammed, Joan of Arc, and others of mark, are depicted. All the characters here

described were, "in figurative language," visited by spectres, who passed through the Ivory Gate, a fancy derived from Book XIX. of the "Odyssey," line 562, and from Virgil, "*Æneid*," VI., l. 894. Under the title of "The Historical Insane" I have myself given, in past years, short courses of lectures on subjects of the same type, taking, specially, as a representative of the class, Ivan the Terrible, John the Fourth of Russia. Maudsley, also, has done much the same thing in his brilliant essay on Swedenborg. Dr. Ireland strikes out more boldly, and in his goodly volume brings before us, with the touch of a master, Swedenborg; William Blake; King Louis II. of Bavaria; Charles J. Guiteau, the assassin of President Garfield; Louis Riel; Gabriel Malagrida; Theodore, of Abyssinia; and, Thebaw, King of Burma. It would be unfair to disclose even a corner of the remarkable delineations which Dr. Ireland has here presented to the world. For analysis of mental phenomena, in health and disease; for epitome of character, as the result of study of analysis; for cleanliness of treatment, if one may say so, these essays are historical; and it would indeed have been a wonder if they had failed to attract all the attention and public favour which has been granted to them. They are not all of equal merit; the one on William Blake, for instance, would admit of being recast with advantage to the author, to the reader, and to the grand subject of discourse, who, as poet, artist, and philosopher, is one of the choicest spirits that our humanity ever produced, or that ever spoke to mankind. But still from cover to cover the pages entrance and hold the mind in abeyance, under the subtle influence of a writer who ventures far because his wide and accurate knowledge combines so well with his sagacity and matured wisdom.

"A MANUAL OF PHARMACEUTICAL TESTING."

[BY BARNARD S. PROCTOR.]

THIS book is intended particularly for pharmacutists, but it is equally useful to members of the medical profession who may wish to test the purity of the drugs they are prescribing. It is a small and handy volume, and the directions supplied for every drug, including all drugs in common use, are simple and clear. A preliminary chapter of thirteen pages on "Manipulation" forms a sound and practical commencement, and a good index finishes the work.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD is exclusively by the Author whose name is on the title-page.

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD should be addressed to the Author at his residence, 25, Manchester Square, London, W.

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, Paternoster Square, London, E.C.

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

REGISTRATION OF MIDWIVES.—*Dr. Reutoul invites attention to the two following important questions relating to the "Amended Midwives Registration Bill," which has been withdrawn from the Commons, and on which he would like at once to take the opinion of the profession:—(a) Do the public and the profession think it necessary to have a class of Midwifery Practitioners other than doctors? (b) If this is thought necessary, what measures should be adopted to see (a) that pupil midwives receive a thorough training; (b) that they be thoroughly supervised; and (c) that the training of pupil midwives does not absorb all the lying-in cases necessary for the training of students and monthly nurses, and bar the young doctor from improving the imperfect midwifery education which he receives when at College?*

August
15th,



1890.

APPENDIX OF ACKNOWLEDGMENTS.

- ALEXANDER, J. G.** "India's Opium Revenue: What it is and how it should be dealt with." By JOSEPH G. ALEXANDER, LL.B. London: P. S. King & Son. 1890.
- BERDOE, EDWARD.** "The Healing Art and the Claims of Vivisection." By EDWARD BERDOE, M.R.C.S., L.R.C.P. London: Swan Sonnenschein & Co. 1890. [A lecture delivered at Cambridge on March 10th, 1890, advocating the cause of those who are opposed to experimentation on living animals. There is a happy peculiarity in this lecture in that it is more argumentative and less violent than others of its kind: and if the question treated of had always been discussed in this manner a great deal of unnecessary friction might have been prevented. The one fault of the author is that he is too absolute in some of his inferences.]
- BRAITHWAITE, JAMES.** "The Retrospect of Medicine." Edited by JAMES BRAITHWAITE, M.D. (January to June, 1890). London: Simpkin, Marshall & Co. 1890.
- BULLOCK, CHARLES.** "An Hour with George Herbert, the Poet of Devotion." "The Two Bishops: A Welcome and a Memory." By the REV. CHARLES BULLOCK, B.D. London: *Home Words* Publishing Office, Paternoster Square.
- CLEVINGER, S. W.** "Spinal Concussion." By S. W. CLEVINGER, M.D. London and Philadelphia: F. A. Davis. 1889.
- COLLINS, W. J.** "Jennerian Hypothesis concerning Vaccine." By W. J. COLLINS, M.D. London: Hansard Publishing Union. 1890. [A republication of an essay published by Dr. Collins many years ago in vindication of the originality of views then considered very incorrect and revolutionary, but which time has largely endorsed as advanced and true. The strength of tone and individuality of the author is well portrayed.]
- COOKE, CONRAD W.** "William Gilbert of Colchester." By CONRAD WILLIAM COOKE, M. INST. E.E. London: Bedford Press. 1890. [A review of the work of William Gilbert rather than of his life and time. The essay indicates that its author possesses a classical knowledge as well as a true respect for the subject of his studies.]
- COSGRAVE, E. M.** "Report on Alcohol and Longevity." By E. MACDOWEL COSGRAVE, M.D., CH.M. Dublin: John Falconer & Co. 1890. [An unanswerable answer to those who have strained to make out that alcohol, as a beverage, does not shorten life. Dr. Cosgrave proves that "the effect of alcohol in shortening life is very great when the alcohol is taken in large quantities." "But," he adds, "it is also apparent that if a large number of people who consume alcohol in moderate quantities are compared with a number similar in other respects, but not taking alcohol at all, the longevity of the latter will be superior to that of the former."]
- DAVIS, N. S.** "An Address on Medicine." By N. S. DAVIS, M.D., LL.D. Chicago: Reprint from Journal of the American Medical Association. 1890. [The distinguished physician, the author of this address, has lived to see the city of Chicago rise from a few houses to become one of the great cities of the world; but his mind retains all its power, trained by experience and wisdom to a degree never better illustrated than in the admirable thesis named above.]
- FINLAYSON, JAMES.** "Plea for a Reform of the University Teaching in Scotland." By JAMES FINLAYSON, M.D. Glasgow: Alex. Macdougall. 1890.
- FOURNET, A.** "Is Bad Sight on the Increase? The Philosophy of Sight." By A. FOURNET. London: Swan Sonnenschein & Co. 1889.
- HELBING'S** Pharmacological Record for July 1890. London: 1890.
- IRELAND, W. W.** "Through the Ivory Gate. Studies in Psychology and History." By WILLIAM W. IRELAND, M.D. Edin. Edinburgh Bell & Bradfute. 1889.

- KIRK, ROBERT.** "A New Theory of Chloroform Syncope." By ROBERT KIRK, M.D., Edin., F.F.P. and S. Glasgow. Glasgow : 1890.
- MACKAY, CHARLES.** "A Dictionary of Lowland Scotch." By CHARLES MACKAY, LL.D. (the late). Edinburgh : Ballantyne Bros. 1888.
- MILLER, F. F.** "On the Programme of the Women's Franchise League." An Address delivered at the National Liberal Club February 25th, 1890. By Mrs. F. FENWICK MILLER. London : Hansard Publishing Union. 1890. [Mrs. Fenwick Miller, who is good reading at all times, is at her best where the claims of her own sex are being considered, as in this address, which readers of both sexes ought to read carefully.]
- MADDEN, THOMAS MORE.** "Physical Education in Relation to Mental Development in School."
 "On the Treatment of Cystitis in Women."
 "Ovarian Herniæ; their Causes, Symptoms, and Treatment."
 "On the Strumous Diseases of Childhood and their Relation to Tubercle."
 "Treatment of Obstructive Dysmenorrhœa." By THOMAS MORE MADDEN, M.D., F.R.C.S. Edin. London. 1890.
- MULLEN, J. W.** "Annual Report of the Hospital Department of the County Borough of Salford for year ending December 31st, 1889." By JOHN W. MULLEN, L.R.C.S. Salford : W. F. Jackson & Sons. 1890.
- MUSSER, J. H.** "Primary Cancer of the Gall Bladder and Bileducts." By J. H. MUSSER, M.D. Philadelphia : W. J. Dornan. 1889.
- PAGET, C. E.** "Annual Report of the Medical Officer of Health for County Borough of Salford for the year 1889." By C. E. PAGET, M.R.C.S., L.R.C.P., D.P.H. MED. OFF. HEALTH. Salford : Jackson & Sons. 1890.
- PALMER, MRS. ALONZO.** "Memorial of Alonzo Benjamin Palmer, M.D., LL.D. By MRS. PALMER. Cambridge, U.S.A.: 1890. [A touching tribute of love and remembrance of the late eminent Professor of Medicine in the University of Michigan, U.S.A., whose name was so widely known and honoured.]
- PARKES, LOUIS C.** "Hygiene and Public Health." By LOUIS C. PARKES, M.D., D.P.H. London : H. K. Lewis. 1890.
- SHAW, C. KNOX.** "Annual Report of the Health, Sanitary Condition, etc., of the Borough of Hastings for 1888." By C. KNOX SHAW, L.R.C.P., M.R.C.S. St. Leonards : Daniel & Co. 1889. [A good and able report on the condition of Hastings and the health of that beautiful and tempting health resort.]
- STANYON, W.** "Holiday Trip to America." By WILLIAM STANYON. London : Simpkin, Marshall & Co. Leicester : Geo. Gibbons & Co. 1890.
- THORNTON, B.** "Operative *versus* Therapeutic Treatment of Strumous Cervical Glands." By B. THORNTON, M.R.C.S., L.R.C.P. London : H. K. Lewis. 1890.
- TIDMAN, E.** "On Public Elementary Schools." Read before the Society of Architects, May 13th, 1890. By E. TIDMAN. London. 1890.
- WALLBRIDGE AND ROWLAND.** "The British Guiana Medical Annual and Hospital Reports." Edited by J. S. WALLBRIDGE, M.R.C.S., and E. D. ROWLAND, M.B. Demerara : Baldwin & Co. 1890.
- WILLIAMS, W. M.** "The Philosophy of Clothing." By W. MATTIEU WILLIAMS, F.R.A.S., F.C.S. London : Thomas Laurie. 1890. [Mr. Mattieu Williams with his usual force and good commonsense sustains the argument originally stated by Count Runford, that porous clothing is the cleanest, warmest, and coolest, as well as the lightest for every-day use. In this he will certainly receive corroboration from all wearers of what is called cellular clothing.]

PROCEEDINGS AND TRANSACTIONS.

- "Proceedings of the Physiological Society." March and May. Nos. II. and III. 1890.
- "Proceedings of the Society for the Study of Inebriety." London : 1890.
- "Twelfth Annual Report of the Society for Training TEACHERS OF THE DEAF and diffusion of the 'GERMAN' SYSTEM for the year ending October 31st, 1889." London : J. Martin & Son. 1890.

- "Twenty-second Annual Report of the New York O.thopædic Dispensary and Hospital for the year ending September 30th, 1889."
- "Report of the Committee of the ANIMALS' INSTITUTE for half year ending December 31st, 1889."
- "Twenty-second Annual Report of the LADIES' SANITARY INSTITUTION." London : April, 1890.
- "Sixth Annual Report of the DALRYMPLE HOME at RICKMANSWORTH, 1889-90." London : H. K. Lewis. 1890.
- "Revue d'Histoire Diplomatique Publiés par les soins de la Société d'Histoire Diplomatique." Fourth year. No. 3. Paris : Leroux. 1890.
- Transactions of the Metropolitan College (SOCIETAS ROSICRUCIANA), with Abstracts of the Lectures read 1889-90. Edited by R. W. FRA. S. L. MACGREGOR MATHERS, VIII, SEC. 1889.

EXCHANGES.

"The New York Medical Journal." "Journal of Chemical Society." "Journal of Society of Arts." "Chicago Med. Journal and Examiner." "New Orleans Med. and Surg. Journal." "Sacramento Med. Times." "Illustrated Science Monthly." "New England Med. Monthly." "Chemical News." "Clergyman's Magazine." "College and Clinical Record." "Glasgow Med. Journal." "Bristol Med. Chir. Journal." "Australasian Med. Gazette." "Journal of Mental Science." "Quarterly Journal of Inebriety." "Sanitary Journal." "Hygiène Pratique." "Med. Temperance Journal." "The Polyclinic." "Asylum Journal." "Journal of Microscopy." "Longmans' Magazine." "Gentleman's Magazine." "Belgravia." "Chemist and Druggist." "Health." "Sanitary World." "Journal d'Hygiène." "Malthusian." "Animal World." "Scientific American." "Bollettina della Società Fiorentina d'Igiene." "Med. Record." "Bulletin de l'Académie de Médecine." "Liverpool Med. Chir. Journal." "Medico-legal Journal." "Hospital Gazette." "Journal Scottish Meteorological Society." "International Journal of the Med. Sciences." "Journal of Heredity." "The Health Journal." "Science Gossip." "The Med. World." "The Photographic News." "Edinburgh Med. Journal." "The Wayfarer." "South African Med. Journal." "Phrenological Magazine." "Med. and Professional Review." "Vegetarian Messenger." "The Analyst." "The China Med. Missionary Journal." Shanghai. "The St. Louis Med. and Surg. Journal." "The Journal." "The Canada Health Journal." "The Draper's Record." "The School Board Chronicle." "Our Day." "The Scientific Inquirer." "Public Health." "The Satellite." "The Band of Hope Journal." "University Medical Magazine." "Illustrated Medical News." "The Journal of Physiology." "New York Abstract." "Murray's Magazine." "Journal of the American Medical Association." "The London Figaro." "The Atlantic Monthly." "Provincial Medical Journal." "Albany Medical Annals." "Occidental Medical Times." "Diet and Hygiene." "The Open Court." "Journal of Physiology." "Journal of Nervous and Mental Disease." "Pictorial World." "Horticultural Times." "Illustrated Missionary News." "Vaccination Inquirer." "Medical Analectic and Epitome." "Journal of Dermatology." "The Photographic Review." "The Medical Record." "Good Health." "Medical News." "Journal of British Dental Association." "American Journal of Medical Sciences." "Drug Topics." "Journal of British and Foreign Health Resorts." "Abkari, Indian Temperance Journal." "Woman." "The Central Review." "Rivista Internazionale d'Igiene, No. 1, Abstract and Index." "Rivista de Ciencias Medicas." "Hazell's Magazine." "Australasian Medical Gazette." "Boy's Own Paper." "University Medical Magazine." "The Friend of China." "Provincial Medical Times." "The Canine Journal." "The Tocsin." "The Hospital." "The Gentlewoman." "The Author."



CENTENARIAN PATHOLOGY.

ON two occasions the opportunity has occurred to me for examining into the physical and mental conditions of persons who have passed over their hundredth year of life. Before the facts go out of memory, forgotten in the multitude of other events and studies, I will devote a few moments to recalling them, so that in the future other observers of similar phenomena may compare what they see and what they read together, should their eyes chance to fall on these pages.

THE CASE OF MRS. H., AGED 103 YEARS.

The first case, that of Mrs. H., came under my notice in the Birmingham workhouse in the year 1864, through the kindness of Dr. Robinson the able Medical Officer of the Institution. The age of this centenarian was completely identified from the register of her birth. She was a native of Birmingham; had lived there, or near there, all her life, and always in the humblest positions. She was an inmate of the workhouse at the time when I visited her, and had been bedridden there for five years. She was of

fair complexion, and had light grey eyes, free of arcus senilis. I should say that she was of nervous or nervous sanguine temperament, and of Celtic race. I could get at no facts as to her parentage or family bearing on hereditary health and longevity. Her habits were said to have been very temperate, and she had never been known to drink any stimulant stronger than tea; but she was a confirmed smoker, and was smoking a short pipe when I came to her bedside. She had never been married, but there was a rumour, the truth of which she always indignantly denied, that in her early days she had given birth to a child. She seemed to have had a singularly healthy life, and was known to have said that she had never been laid up in the whole course of her career until she became bedridden. She rarely complained of pain, and evidently knew little about pains of any kind. Her appetite was surprisingly good, and her desire for her meals sharp and decisive; but she ate whatever was put before her without demur, provided it was of sufficient quantity, pleasant taste, and well cooked. She also digested her food with ease, and enjoyed her pipe after each meal. She slept well, going to sleep every night, and waking early in the morning, with an occasional short sleep during the day. Her mental powers were failing her; the recollections of the past were limited, and her memory of the present was exceedingly short. She was at times irascible, and when persons near her were speaking together, as if about herself, she usually concluded that what they said related to the supposed delinquency of her early life, upon which she was led to give to them, in a short stereotyped sentence which I cannot repeat, a bit of her mind. Her physical powers were low, but she could sit up in bed without assistance, and could help herself to all foods and drinks placed within her reach. Her

physical failures were in her muscular powers generally, perhaps most marked in her lower limbs; but, there was no actual paralysis of motion or of sensation.

OBJECTIVE PHENOMENA.

It is a matter of regret that I did not take the temperature of this centenarian, but I learned that she never complained of cold, and that her extremities and body always felt warm to those in attendance. I examined the *thoracic region* with the greatest care. There was good resonance, on percussion, over the chest everywhere, and on auscultation a natural vesicular murmur was distinct at every part. The murmur was puerile, similar to the murmur in a little child, due probably to the circumstance that the chest walls were thin and somewhat shrunken. There was no trace of bronchial *coo* or *râle*. She could fill the chest readily, and the movements of respiration were even and regular, eighteen to the minute. The sounds and movements of the *heart* were as natural as those of respiration. The motion was regular, and free of any approach to intermittency. The cardiac sounds had correct measure between the first sound, the second sound, and the pause. The quality of the sounds was pure; there was not the least harshness or coarseness; no accent, no sharpness. The action of the heart was feeble and quick, as is common in old age, eighty-five to the minute. Between the respiration and the circulation there was natural harmony.

The *bowels* acted, naturally, once every day; the *kidneys* acted regularly, and the *urine*, which Dr. Robinson had tested, was reported to be free both from albumen and sugar, specific gravity 1020, reaction acid. The *skin*, pale over the whole of its surface, was shrunken and wrinkled and leathery, loosely covering the muscles,

which were much shrunken—one of the most definite signs of extreme old age—but soft of texture, and free of roughness. Her *senses* had become much impaired. Her sight was moderately good, enabling her to recognize persons and objects around her; but her hearing was defective, and indeed to slight sounds she was altogether deaf. The expression of her countenance was not suggestive of her real age; she might have passed for eighty, and had she been furnished with artificial teeth, to remove the collapse of the cheeks and lips incident to the loss of her natural teeth, she would have passed for a woman of seventy-five.

Mrs. H. lived for a period of two years after the time when the notes of her condition, rendered above, were taken down. She gradually grew weaker, and finally succumbed to a slight attack of bronchitis following upon a cold, one of the most common modes of dissolution in persons of very advanced life, a kind of natural euthanasia with death commencing in the respiration—*anæsthesia veterum*.

THE CASE OF MRS. A., AGED 102 YEARS.

The second centenarian to whom I have to refer came under my observation for the first time on August the 22nd of the present year. For this observation I am indebted to my learned professional brother, Dr. Crespi, of Wimborne, Dorset, who was so good as to arrange for my visit, to accompany me, and to render me most able assistance. He also accompanied me on a second visit on September the 18th of this year.

Mrs. A. was born on August 10th in the year 1788. This is recorded in the family register, in an old Bible, and is confirmed by the register in the Minster, which

shows that she was baptized on February 13th in the year 1790. The baptism was eighteen months after birth; but the circumstance in no way invalidates the private record, since it was common in past days to delay baptism for many months, and sometimes for years. As in the previous instance, Mrs. A. is bedridden. She met with a fall about ten years ago, or fell from sudden failure of muscular power, on her left side, and since then has never walked without assistance. She is of fair complexion, has bluish-grey eyes, is of nervous temperament, and I should think of Saxon blood. On one side of her family she inherits a tendency to longevity: her father died at eighty, and his mother at one hundred and three. Her mother died at seventy-two, but her mother's mother died in early life, in giving birth to her mother. She married whilst she was young, and has had five children, two sons and three daughters; the two sons are dead,—one from an accident in his twenty-sixth year, the other at forty-five from disease, the nature of which is not known. The three daughters are alive, one of them, now in her seventieth year, being present at the periods of my visits; she is the third child of Mrs. A., is in fair health, rather pale, but strong for her years, and inclined to be corpulent.

From her birth Mrs. A. has been a total abstainer from alcoholic liquors, not from adhesion to the principles of total abstinence, but because she never, at any time, had a liking for the alcoholic class of drinks. She had beer and other alcoholic fluids at command whenever she liked, but preferred water or tea, and found total abstinence both agreeable and healthy. She never smoked. During a long period of her life she was engaged in domestic service, and was of active and industrious habits. During the whole of her active life she has been remarkably free

from disease ; indeed, until she met with the accident or fall at the age of ninety-two, she did not know what it was to be ill. She took to her bed on that event, owing to the want of power in her lower limbs, with loss of power in her left arm ; but with these exceptions she has been in good health and spirits. She complains of pain on movement, and is nervous on being lifted up, but whatever is felt is soon over when she comes to rest. Her appetite is excellent, as it has been all through her long career, and at present she takes as many as four meals a day, digesting them with the most perfect ease. She sleeps fairly well during the night, and indulges in a nap during the day, but she is not what would be called a heavy sleeper. Her description to me was that she had always been a good sleeper, and could sleep at any time if she were not disturbed, but that she was easily wakened up. Her features are small, and cheeks sunken from loss of teeth, but her expression is intelligent, and at moments vivacious. She is deaf, and is obliged to use an ear-trumpet when she is spoken to, but when she becomes accustomed to the voice that is addressing her she understands what is said, and is ready with replies to questions that may be put to her. Her voice is fairly strong, her words plainly rendered, and the sound, although a little shrill, not harsh. Her memory is good in regard to events of her early life. She remembers the great news of the victory of the Nile, and many of the names and events of the first ten years of the century. She also recalls events of a striking kind that have taken place, at different periods, all through the century, and I was told by those about her that her memory of things that now happen around her was not altogether impaired. The recollection of her husband's illness and death a few years ago remains vividly, as well as the particulars

of his life and his mode of life. She takes interest still in some current events, and likes "being read to," her two favourite books being the Bible and Old Moore's Almanack. Her sight, she told me, was failing, but she could see "ordinary well," and could easily discern those she knew from those who were strangers. She could see without glasses everything that was about her; and I observed that when she looked for anything she wanted,—her ear-trumpet, handkerchief, or other object near at hand,—she detected it as quickly as if her vision were in the best condition. Her senses of smell and taste are unimpaired, and her sense of taste acute. The common sensibility of the skin is perhaps rather lower than natural, but the failure is so trifling I should not like to be too positive on that point.

OBJECTIVE PHENOMENA.

The objective phenomena in the case of Mrs. A. come next under notice. The opportunity for making them was too important to be lost, and I therefore took the utmost care to collect and write down the facts with the closest detail. They afford in many respects an entirely unique record.

Temperature.—The temperature of the body is a little lowered. It has been taken by Dr. Crespi on several occasions subsequently to my visits. The temperature under the tongue is very uniform, and presents a mean of 98·2° Fahr.

The Pulse.—We took great pains to observe the pulse. As is usual in the aged, the pulse was small, quick, and very easily compressible. It averaged on August the 22nd 98 beats per minute. I was able, on my second visit, to take the pulse both with the sphygmograph and the sphygmo-

phone; the first time, I think, such readings have been supplied by a centenarian.

The results, exceedingly curious, were not obtained without considerable trouble, for the radial artery on each side moved beneath the skin with so much freedom that it glided from the cushion whenever pressure was put on it. Moreover, the slightest pressure sufficed to stop the pulse as if it intermitted, which was not the fact, the pulse on the opposite side being free of intermittency. By patience I got at last the tracing copied below.

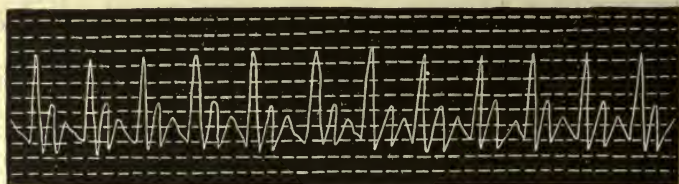


FIG. 1.—Pulse tracing of a person 102 years old.

From the natural tracing this centenarian pulse varies in the following manner. The ascending, straight, or impulse line, systolic line, passes through five and a half degrees on the scale instead of four degrees and a half; the first descending line, showing the decline of the ventricular pressure, passes down through six degrees at a very acute angle; the second short ascending line passes through three degrees, with a little curve, as if the recoil of the artery which produces it were slow and prolonged; the third short descending line, caused by contraction of the artery, takes three degrees, followed by a sharp line running up two degrees, from the aortic rebound; a final short, oblique descending line indicates the subsiding arterial wave produced by the rebound. It will be seen that the six natural events of the pulse

are retained, but are much changed in quality; the impulse of the blood-current is strong, the filling of the artery free and its subsidence gradual; the contraction of the artery and its subsequent relaxation is prolonged, but the action of the aortic valves is good. Another peculiarity relates to the number of each set of events per minute, compared with the natural standard. In this pulse the events were repeated twelve times compared with nine of the natural pulse on the same length of tracing in the same time. A natural pulse, beating at 72 per minute, would have represented nine events on the tracing; whereas this centenarian pulse, beating at 96 per minute,—two beats lower than when it was counted at my first visit,—represented twelve events.*

The nearest pulse reading to this one I have ever met with is figured in the subjoined drawing (fig. 2). It is a tracing of the pulse of a man rendered prematurely old by alcoholic indulgence. The tracing was taken during the first stage or degree of temporary excitement from alcohol.

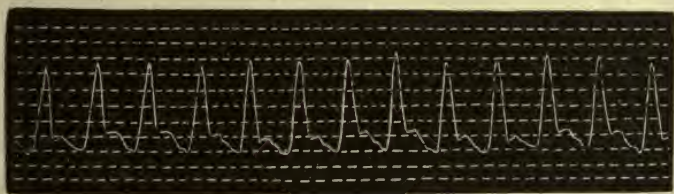


FIG. 2.—Pulse tracing of a person prematurely aged from alcoholic indulgence.

In summary, the reading of the centenarian pulse

* The pulse may possibly have been somewhat excited during our visit, by the presence of two strangers, but it varied not more than six beats per minute all the time we were there, and from 94 to 98 may be taken as the normal. This tallies with observations I have previously made on the rapidity of the circulation in old age.

shows an unusual pronouncement of each part of each event constituting a complete pulsation, with rapidity of pulsation, and feeble resistance.

The sphygmophonic record was much more easily taken than the sphygmographic. The first or impulse sound was shorter than natural; the second sound, incident to the arterial recoil, was lengthened; the third, produced by the back current of blood from closure of the aortic valve, was prolonged. The whole corresponded well with the written record, conveying the intimation that the heart continued in fair working order, but that the resistance to the transit of blood in the extreme parts of the circulation was considerable, and that, virtually, the circulation was being shortened, and made to approach somewhat to the shorter circulation of childhood, less the resilience common to that period of life.

The Thorax.—A careful examination of the thorax, for evidence of the condition of the lungs, revealed a less perfect state than was discovered in the first case detailed above—case of Mrs. H. To percussion the chest was clear over every part, and the natural respiratory murmur was audible in all parts; but in the larger bronchi there was a soft mucous râle, and in two places in other parts, notably in the base of the right lung, laterally, and in the base of the left lung anteriorly, just below the apex of the heart, there were slight bronchial râles, which, from the general history, I should presume to be signs of dilatation with thickening of the bronchial surfaces. Owing to these conditions a chronic bronchial cough with bronchial expectoration, as is so common with the aged, has been present for a considerable time.

The Digestive System.—The digestion was remarkably good. The tongue was perfectly clean, healthy, and of natural colour. Food was partaken of freely, and there

were no symptoms of dyspepsia, no oppression after food, no eructations, no pain, no uneasiness. The action of the bowels was once daily, and quite natural.

The Renal Function.—The function of the kidneys was carried out in natural course, as a rule, but now and then there has been a tendency to diuresis, particularly during the night. This I was led to infer has been due to the imbibition of an excess of watery foods, and not to disease. The specific gravity of the urine, determined from specimens forwarded to me by Dr. Crespi, was 1020; the reaction was acid, the colour natural. There were no indications of albumen, none of sugar.

The Skin.—The function of the skin was not free, but the surface was not actually dry. As a subjective fact the surface of the body was warm, and the complaint was rather of undue warmth than of coldness. The equality of the warmth of the body, even to the extremities of it, was well preserved.

The facial expression was good for one so old, giving at first sight the idea of a person about eighty years of age, which longer observation rather confirmed than removed. The hair was greyish white, but not of that silvery hue which would be expected; in a word, I have never seen a person of eighty years who looked younger than this veteran of one hundred and two years. Her expression was free from sense of brooding, care, or apprehension; and I should imagine that the dread or idea of death finds in her mind no place, or, at all events, suggests no anxiety or fear. The appetites which sustain the vital acts connected with self-preservation,—or, what some would call the instinctive faculties, which the human animal holds in common with his lower earth-mates,—still remain intact; the emotions are subdued; the reasoning powers are in abeyance, and probably are irrevocably lost.

COMMENTARY.

NORMAL DURATION OF LIFE.

In the two individuals whose histories have been recorded above we have examples of persons who have nearly attained the anatomical duration of human life. I mean the anatomists' calculation of the full period of human existence. We are indebted, I believe, to Fluorens for first formulating the duration of life on anatomical grounds, but he borrowed from Buffon, and Buffon from some who preceded him. Buffon said that the duration of human life depends neither on climate, food, nor variety; it depends on nothing that is external; it depends only on the internal constitution, and upon the intrinsic virtue of our bodily organs, everything in our bodily organs being subject to fixed laws. Each species of animal, he contended, has not only its distinct form, but each has, also, its determinate period of gestation, and its particular duration of growth; and if all these things—form, gestation, growth—have their regular and determinate duration, why should not life have its own? After this expression of view Buffon explained, as a fact which seemed clear to him, that the total duration of life can be measured, in some degree, by the period of growth; and he proceeded, in a general way, to calculate duration of life from growth. According to Fluorens he failed to give a correct estimate, because he did not know the certain sign that marks the full term of growth; a sign which Fluorens discovered in the union of the bones with their epiphyses. As long as the bones are not united as one bone with their epiphyses, the animal grows; when the bones and epiphyses are united into one, the animal grows no more.

The relation pointed out by Buffon was very near the truth as to growth and duration of life, when he put the duration at six to seven times the period of growth; but the true duration is five times. Thus as in man the duration of growth is twenty years, and as five twenties make a hundred years, the natural duration of human life is one hundred years, which is in accord with the ancient saying, "yet shall his days be a hundred years."

So many examples of centenarians are now occurring, we may assume with strict fairness that the estimate of one hundred years is near the mark, and that under proper and favouring conditions the age of one hundred years might come readily into the order of nature. The two cases which have been reported above do not, however, throw any very clear light on the cause of the longevity, although they confirm what Buffon has said, that duration of human life depends neither on climate, food, nor variety; on nothing, in fact, that is external, but entirely on internal constitution. In the second of these cases heredity played an important part; a circumstance which has been observed in many previous illustrations of a similar character, but in what way heredity plays its part is not particularly clear. Both cases evidently indicated a transition from physiological to pathological states, for the bodies of neither of these patients could be considered healthy. But in their pathology there was manifested this marked feature, that the decay, if decay be the correct term, was uniform throughout all parts, so that the body was failing universally, each organic structure becoming enfeebled with its fellows, the general effect being to produce a common enfeeblement of mind with a retention of the purely animal functions.

SECOND CHILDHOOD.

The term "second childhood" is commonly applied to the period of old age; and it is most singular to observe how correct the term is. It is correct with one exception, namely, that in the first childhood body and mind emerge into a stronger vitality, whilst in the second the tendency is to pass on to extinction of vitality—death. The mind in old age becomes again childish; the reasoning powers decline, while the animal powers remain, and in some respects are intensified. It is noticeable that in both these centenarians the digestive functions and the desire for food remained. I have observed this in other persons advanced in life, and I believe that whatever else may be at work sound digestion is the basis of long life in a physical point of view. In first childhood we see also the value of good digestion. Physically the analogy of the first and second childhoods extends further. The pulse quickens, the course of the circulation becomes shortened, and at last the manifestations of living action concentrate in the vital continuance of the organic or vegetative existence, in the retention of working power of the digestive organs, the lungs, and the heart. The involuntary powers, which once unconsciously ministered to a conscious life, continue in their action, but leave the higher mental functions behind them, dead and gone.

The question is often asked, whether it is desirable that human life should be prolonged to these advanced periods. The answer to this question appears, to my mind, exceedingly simple. The whole is relative. There are thousands of persons who at seventy years of age are as enfeebled as the two whose histories I have supplied, and there are thousands upon thousands even at sixty who have not the vital power or energy which

these centenarians possessed when they were ninety. Why should this be? Why should any one be practically a hundred years of age thirty years before the appointed time? I, for my part, see no reason for it except ignorance, and if it be an advantage for the body to attain its fullest range of life, and if it be the natural law that a hundred years shall be attained, then such facts as have been now collated ought to be multiplied by every medical observer until the conditions leading to the production of complete old age, terminating in the natural euthanasia, should become the regular course of events in human existences.

From the natural prolongation of life to its allotted term much good would spring. If it were proved by daily observation of natural facts that life could be prolonged under simple precautions of a health-giving character, the hasty devil-me-care kind of sentiment which so many persons not only profess but practise, would cease, and all would seek how to have, hold, and enjoy the completed blessing of life which has been ordained for mankind, but can never be enjoyed so long as the existing short life leads to contempt of life, and produces, beyond anything else, the so-called logic of despair. From the sense of the necessity of good conditions and good environments would come general and good sanitation, an era of perfect health, and, with that, an era of happiness which has never been known in historical records that can be relied upon as absolutely true; for health and happiness go hand in hand so surely, that the man who is unhappy is not in health, whatever his appearances, of a physical kind, may be. The possession of the knowledge that death has no sting when it comes at the right time, and that death would be as unconscious an act as birth if the proper ordination were

fulfilled, would give to the universal human life a relief equivalent to that which is experienced by the lower creation. The web of life would be spun in order, and, perchance, with a better effect than if the individual life were continued on without the event of death at any period, since death would become real rejuvenescence from new stock, destined to pass through an order of progress which would leave each generation ripened into a natural maturity. The grandest advantage of all would, however, lie in the circumstance that the knowledge which the mind of man may acquire in the present imperfect span of vital and mental activity would be allowed to grow into wisdom. At forty years of age man ought not to consider himself as more than simply stored with the elements of natural knowledge. Well trained, his mental faculties might then be charged to the extent of his capacity. The application of that rich store would, nevertheless, remain as a work to be achieved. The knowledge would feed the days to be applied to wisdom, and mankind, from matured wisdom and experience which could easily be in activity forty years more, would be guided into paths of peace and prosperity we, in this helter-skelter, short-minded, short-lived day have no conception of, no appreciation.

We Esculapians should ponder on these things. Nature tells us it is no dream of the dreamer that human life may exist for a hundred years, and then cease in euthanasia. She says this is her decree, and that nothing except the ignorance of mankind prevents that decree being carried out. We are the priests of nature, and her perfected work is our practical vocation.

MEDICAL TEACHING, OLD AND NEW.

AN OCTOBER STUDY.

IN my autumnal vacation this year I had the good fortune to meet, accidentally,—at Bishopstoke railway station,—on the morning of the first of September, a friend whom the whole profession of medicine sincerely loves and respects—the Regius Professor of Medicine of Oxford, Sir Henry Acland. We had some little time to wait for our respective trains, and our conversation, commencing with a reference by Sir Henry to the life of Ambrose Paré, drifted into the subject of medical education of the present day, on which he is so admirably informed, and still so deeply interested. Our accidental consultation, if it may be so called, cut short—much too short—by the train which waits for no man, left on my mind some of the thoughts woven into the present article: thoughts which chime in well with the October season, when new blood is being so largely infused into the old body of physic.

THE OLD AND NEW NOVITIATE.

The first thought that comes up relates to the different kind of student who is taking to physic in this day compared with him who was playing the same character fifty years ago, a date Sir Henry grasped entirely in his

retentive memory, and which I can touch within two or three years. The student then would have just left his apprenticeship, or, to use a better phrase, "would have just finished his articles." He had, nominally, served a master for five years, the said master being of necessity a licentiate or member of the Apothecaries' Company, in England, or a licentiate or member of one or other of the Corporations in the sister kingdoms, from whom articles were required; but the rule was becoming, in practice if not in theory, a little relaxed. The student was said to serve, "after the manner of an apprentice," for five years; but under that convenient reading two years out of the five could be spent in the hospitals and in attending the lectures at the medical schools, by which the period of medical education, from the commencement of the articles up to the day of examination, was abbreviated to six years: three years for articles, and three for the school and hospital curriculum.

The student, in the days of which mention is made, came from a doctor's home into his hospital studies, bringing with him, not unfrequently, introductions from his old master to his new teachers; he became a Guy's man, a St. Thomas's man, a Bartholomew's man, an Edinburgh man, a Glasgow man, or some other man, as his master had been; and, he carried with him a sort of fore-knowledge, from what he had heard already, of much that he was about to learn for himself, and the traditions of the "old place" to which he entered.

This is not the first time I have expressed an opinion on the quality of the apprenticeship or article system as a part of medical education. I repeat, emphatically, an opinion often before given, that the system was a good one, both in theory and in practice. It was an excellent introduction into medicine. In a profession

like ours, knowledge, scientific knowledge of the highest class, is necessary ; but something more is wanted, namely, a knowledge of those who are to be treated, of men, women, and children, the two latter especially. What is the mental as well as the physical constitution of a practice? What is the method of work that shall win the confidence and, it is not using too strong an expression, the affection of the constituency of a practice? What are the little forms and niceties which the doctor shall observe with the sick? What shall be the manner of entering the sick-room, of sitting down by the bedside, of putting the questions, of making the necessary observations, of speaking to the sick so as not to alarm them on the one hand or trifle with them on the other? These are learnings of greatest value. Again, there are social questions which form part of the medical career. What is the proper social attitude for the practitioner to take in the community in which he finds himself? How is it best for him to accept and tender hospitalities? What accomplishments other than those which are purely professional may be wisely entered upon? What is the best method of laying out the work of a day? What are the peculiarities of union practice, club practice, private practice? Above all, how is obstetric practice, so imperative in its demands, to be allied with the other lines of practice?—Or, as I heard it once observed, how is “the call of the night bell” to be made so familiar that it is always properly responded to, with least fatigue to the practitioner, and fullest satisfaction to his patients?

These were points of practice which came, naturally, to the youth who was taking his first lessons in a medical career in the old times. They were most important lessons in that career. They were lessons that could

not be acquired too easily; they were the form and body of the work that was to come. But there was a great deal more than has been stated. As a matter of fact the student picked up an astonishing amount of good practice, in a detailed and steady manner that cannot be imparted to masses of students gathered together in a class. The article novitiate was alone with his master, without any distraction of sight or of hearing, without any diversion from the one thing that had to be learned, the one thing that had to be done. Was it a tooth that had to be extracted, a bandage that had to be applied, a dislocation that had to be reduced, a fracture that had to be set, a catheter that had to be passed, a chest that had to be percussed or auscultated, a prescription that had to be written or dispensed? There was everything and every instruction neat and ready to his hand. He was made familiar day by day, in an insinuating, steady, systematic form, with all that was going on. He might be too young to attend an obstetric case, but he would be certain to pick up from his master, in the course of conversations and descriptions, a large amount of information even on obstetric subjects. Moreover, before his term was actually passed it would almost of necessity happen that some obstetric practice would come under his hand, and an art be commenced, the strict rules of which a practitioner cannot learn too early, and in which manual dexterity cannot be too quickly acquired.

I can remember the time when this kind and quality of medical education, in the domestic school of medical education, was considered of the supremest value; and I can testify that the value put upon it was not one jot over-estimated. I am quite sure, for my own part, that in technical details I learned far more under articles than I did under hospital training, although no hospital

in the world could have afforded better opportunities for practice of every kind than that to which I became attached, the Royal Infirmary of Glasgow. In that Infirmary, practice of every kind was obtainable, under teachers the most devoted, the most capable, the most industrious. But how different was the method of learning with thirty or forty fellow-students around me, compared with the quiet, concentrate lesson I should have taken from the also able teacher in the domestic school of education I had left, where every little detail, even to the insertion of a pin into a bandage, or the selection of a needle and thread, would have been subject of moment, and where the interest taken in the comfort and welfare of the particular patient would have been, for many days, the one absorbing care!

But domestic preliminary education has died out in our times. The art of dispensing medicines, even, has become a form and little more. Students who have not yet learned to swim in the ocean of physic take to it now with a header, and not unfrequently are persuaded to dive down straight to the bottom of an ocean with the surface of which they have scarcely a trace of familiarity. Why in a practical art like ours this should have come about is beyond comprehension. Perhaps a sort of rebellion first arose against the term "apprentice," a foolish prejudice against commercial as opposed to, and inferior to, professional life. Then a rotten idea was floated that until anatomy was mastered no kind of practical teaching was of any use; followed by another idea, equally rotten, that until chemistry was mastered, the study of the action of remedies was of no avail. So, gradually, the domestic novitiateship, the soundest basis of solid medical construction in theory and in practice, has either seen its last days, or is awaiting its revival in a newer and

perhaps more comprehensive form. The modern student is an academician, not a medical novitiate; he has matriculated in everything except physic.

THE OLD AND NEW TEACHER.

By the new plan a most serious loss is being sustained in the work of the medical student, and more serious still in the work of the qualified practitioner. In the old days every practitioner was a teacher, a master as well as a practitioner, an advantage which all good teachers recognise as a peculiarly useful combination even for their own advancement in knowledge. The man who is teaching must be learning, since he must receive before he can give. Moreover, great pecuniary losses have followed the giving up of the domestic part of the curriculum. For the three years' articles the practitioner was usually paid a fee of from one to two hundred guineas, just as a solicitor—shrewd fellow!—is still paid for the artichoke pupil he takes charge of. There are at least five thousand practitioners of medicine in the United Kingdom, each capable of conducting the preliminary education of young physic, and each of whom, under proper regulations, might command every year a minimum fee of one hundred pounds, without depriving the hospital teachers and school lecturers of a single penny. One hundred pounds multiplied by five thousand reaches the sum of half a million, and that, at lowest computation, represents the pecuniary loss now sustained by the profession at large. It is much; but it is a trifle compared with the good and useful work that is lost both to master and pupil.

At the last election of the General Medical Council I did all I could to present this argument. To the

argument not one reasonable objection was raised; and outside the ranks of the profession it was received with favour. Inside the ranks, however, I must admit, it found little favour; nay, by some it was considered an insult to propose to professional men that they should be expected to take pupils. But I stand still to my guns, and affirm that instead of insult it suggests the highest duty of the practitioner of medicine. I affirm that there are hundreds of men in practice, who scarcely know how to live from day to day on the fruits of practice, who, if they turned their excellent abilities in the right way, and gave up the foolish notion that it is *infra dig.* to teach, might, with the highest credit to themselves and benefit to mankind, be conducting small preliminary schools of medicine, and communicating *secundum artem* their best knowledge and skill to those who are to follow them in their career. There is also no reason why the modern practitioner should not go farther in this direction than his predecessors. Why should he not include in his teaching the anatomy of the skeleton and all other parts of the animal body that can be taught from comparative specimens? Why should he not teach histology and microscopical research generally; why not include elementary physiology; why not elementary chemistry; why not principles of hygiene? A museum and work-room attached to the house of every man in practice would be a convenient addition, and work in it a continuous delight, progress, and profit. Why, too, should not teaching, so conducted, be accepted by the Medical Council as certificated teaching, and as a part of the modern curriculum? If I were a young practitioner in country practice, I should not, even as things are, hesitate a moment in commencing to seek for pupils, and undertaking this preliminary

work : it were far better than trying to add to a household the inebriate patient, the insane patient, or the semi-friend—semi-lodger, so often sought after.

THE PRESENT AND PAST CURRICULUM.

The current curriculum is, to my mind, inferior to that of the old times. Sir Henry Acland spoke with deepest regret on some parts of it, and of its general bad effect from the strain it enforces. "No human mind," he said, "could take in profitably and permanently all that is now attempted to be forced into it in the short time between preliminary and final examinations." I quite agree with this true and wise view. We are proceeding now purely on the system of surfeit, and with the necessary results incident to that pernicious system. We are proceeding as if we fancied that, when the academical life is ended, and when the diploma is obtained, the end of medical training is accomplished. The poor man, wearied of his studies, feels and thinks, "Now I am through I will never touch another book of physic." Such too often is the pitch of weariness and disgust to which the mind is brought. "What I learned for pass examinations has in many instances gone from me as if I had never learned it," is the expression of many a man who has been in practice for two or three years. Practice is found to be so much easier a matter than the work by which it was preceded, that desire to resume the fore-gathering labour seems like an absurd effort. Inducement for continued study is tainted at its source by the dead study which it recalls, and the best advances are cold because the steps by which they have been arrived at are disliked or forgotten. Yet, really, in the science and art of medicine, of the best sort, the whole of life ought to

be the student life. The student in his simple way should always be the practitioner ; the practitioner, in his simple way, should always be the student.

As I write the lives of the great men of the past in these pages, the thought will constantly intrude itself : What on earth would these men have been under our present system of education ? They never could have been what they became. Vesalius could never have completed his magnificent anatomy ; Harvey, the discovery of the circulation of the blood ; Gilbert, the first steps in electricity ; Leuwenhoeck, the first microscopy ; Boerhaave, the first organic chemistry ; Paré, the reformed surgery ; nor could any one of the twenty-eight who have been before us have won a name or fame at all approaching to what they did win, under the present method. Whilst, some others not yet named,—John Hunter, for one example,—would never have had any place at all, or chance of gaining place, with the fetters of the modern system of education dangling before their eyes ere they commenced their work, and chafing them into hatred of it afterwards. Mentally galled, and crippled, they would have fretted simply into commonplace, or have fled for good from a slavery that had neither inducement, pleasure, chance, nor hope.

It may be urged that the medical education which has permitted the highest medical development in particularly gifted men is not the best for the common wants of the community, and that genius itself must be kept within some restriction in order that general advance of practice shall be for the general good. It may be urged that a decent mediocrity is preferable to the position prior to 1815, when a few men of supreme quality stood above a large majority of practitioners who had no certified qualification for the important art of cure. In this there

is common sense, and we may feel pretty confident that before the passing of the Act of 1815, which gave the apothecaries so much power, and which for the first time established in England an examination for the practitioners of the kingdom, there was abundant cause for the Act. But between that first stroke of reform, and the present extension of it, there is as much difference as between a well-organised, constitutional government and one in which constitutional regulation has passed into organised tyranny. The first examinations carried with them proof of sufficient practical knowledge to permit the young practitioner to go forth armed with as much skill as placed him on a professional footing with his compeers already in practice, less the experience they had acquired, and which was for them a title of precedence gained by a longer course of work and observation. There was no pretence of doing the impossible act of making every new batch of qualified men better practitioners than their predecessors, by the plan of forcing a higher standard of theoretical education, to be tested by a more stringent examination. Parchment *versus* practice had then no place; but the examined man, having satisfied an elementary and sufficient curriculum, and having submitted successfully to a common-sense examination, was allowed to go forth, uninjured mentally, and free to win his way in fields of improving knowledge: a practitioner by human, a student by natural, law.

The present system, carried to its extreme degree, is pressure into density; perpetuated for one or two generations it can have but one result, the entire annihilation of all originality in medicine, all love for it as an art, all true progress in it as a science, and ending in its transformation into a common mechanical automatic mediocrity. In time, as the demand goes on for more and more

pressure, the free school of medicine will cease. Grinding will be the one art of the teacher, an art brought to an edge that will cut up the finest brain into the commonest pulp the world can tolerate.

The reaction into a sounder state will in time come, out of sheer necessity, not from the profession but by the governing power, by State interference. It will be found that, for every necessity and every emergency, the high-pressure system does not work; then the State will once more begin to consider the propriety of instituting a board of examiners, that shall give one qualification based on a moderate test leading to a legal admission to practise in any and all branches of medical art, and that shall be the one compulsory portal into the professional body. All will then again be practically free; teachers will be free to teach what and where they like; students will be free to learn when and where they like; and the best teachers and the best learners will acquire, by their superiority, the first and best places, as in days of yore. The change implied in this new movement will have the most useful effects in the freedom which it will offer to intellectual gifts, and in the opportunities it will render to genius. A great benefit will be tendered to education, and to educators. Professorial work under this rule will again become distinguished, and to be a master in the art of teaching will be a mark of the highest rank. Teaching will be philosophical and learned; the professors will devote their lives to the preparation of their work, will think out their lectures well before they read them, will construct them logically, and deliver them in clear, forcible, classical language. In a word, medical teaching will become an art and medicine a science.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

CYCLING FOR THE INSANE.

T the Psychological Society's meeting at Bethlem Hospital on May 15th, 1890, a paper was read by Dr. C. Theodore Ewart on the subject of "Cycling for the Insane," followed by a debate, in which several members took part. The learned author of the paper expressed views which may be epitomised under three heads. He conveyed his opinion that the insane could, under proper supervision, be taught to cycle; that the exercise would be to many of them a good and pleasant diversion; and, that it would be an exercise which, by promoting active cutaneous excretion, would relieve the body of organic products, which, by their presence, affect injuriously the mental functions. It was satisfactory to me to have it in my power to support all these positions, though I ventured to suggest that the patients upon whom the experiment should first be tried ought to be those of the hypochondriacal family, whose depression and melancholy are probably due to the accumulation in the nervous centres of sulphur compounds, like mercaptan, which we now know may exist in the gases of the alimentary canal as products of imperfect digestion, and which are found to have the power of inducing melancholic depression when they are

inhaled for experimental purposes. In cycling the free cutaneous elimination produced is directly useful for removing products of the nature described; and in cases of depression lying within the borders of sanity, there cannot be a doubt that cycling often proves of the most signal service. In other instances of mental disease in which there are maniacal symptoms, there are some reasons for doubting the efficacy of the proposed treatment, but this *sub judice*, and not in opposition to the trial of the experiment.

On one point I felt it necessary to differ from Dr. Ewart. He suggested that the exercise should be conducted amongst the insane by placing them on a compound machine, in which a number of machines are attached to each other in line, as in the Victoria tricycle. By this means he thought the attendants could ride with the patients, and could do all the direction and driving, leaving each patient to perform a moderate amount of pedal work and nothing more. My experience of cycling was that this method would lead to considerable danger; that it would be a most difficult art to steer under these circumstances; and that the resolute obstinacy, folly, or fear of one rider might often involve the whole line of riders in the common danger of a fall. I have thought this point over since the debate, and the same feeling as to the risk involved is confirmed; but the recommendation that for the service of the insane light strong tricycles, of the Crippler type, should be used, and that each patient should be supplied with a machine of his own, may perhaps be modified. It may be advisable, in some instances, to provide a machine on which an attendant and three patients could ride. This would be manageable, and might be constructed in a form that would enable the attendant to have his companions in front of him and under constant observation. The

construction of a cycle of the kind named is well worth the attention of our best manufacturers, who may be certain that, if once a good and practical machine comes into use in the asylum for the insane, it will soon be in great demand.

LARGE SUPERFICIAL VEINS OF THE EXTREMITIES.

LARGE superficial veins of the upper extremities, of the hands especially, are always a sign of constitutional feebleness. I have never met with a powerful man or woman who presented in early life large and full veins in long lines of view, except to notice that those who have presented them have been of feeble constitution. The schoolboy with full veins ought to be carefully observed by his master as one whom he may be sure is incapable of any very laborious mental task, although his physical powers may be fairly good. The full straight vein may mean a comparatively feeble right heart, an auricle and ventricle on the right side which offers an indifferent contraction, and therefore indifferent resistance to the onward course of blood to the heart ; or it may mean a venous obstruction in some large vascular organ. In large veins the valvular structure of the veins becomes imperfect in action, and contrasts, in the most marked degree, with the firm "knotted vein," which in animals, like the high-bred horse, is so characteristic of strength and firm development. In sound health the veins of the hands are scarcely visible until after middle age ; and in the most robust the appearance of full distended veins on the back of the hand must be accepted as the primary indication of advancing age, the first descent into the fourth, towards the fifth and last period

of human existence. In other words, the fully matured period has reached its height, and the declining period is coming into view.

I have noticed that some diseases are followed by development of large veins, a sign of languid circulation. This is most peculiar in the true malarial affections. Persons who have suffered from tropical malarial affections usually present the large veins at the back of the hand, extending in some instances along the arm. The sign is so special in these cases I cannot remember an instance of the kind in which I have seen it absent. It is due in these patients, I should presume, to long-continued obstruction of the venous currents in the large vascular organs, like the spleen, and to continued tension of the superficial veins, by which they become permanently dilated.

The large vein is, again, a sign, in some instances, of cardiac diseases, usually of disease affecting the right side of the heart, dilatation with thinning of the walls. It is also witnessed in cases of venous obstruction in the lungs, and notably in asthma.

In a hygienic point of view the observation on the veins of the hand is important for the strong as well as for the weak. When large veins are manifested the intimation is given that two precautions are needed: the first, that strain on the physical and mental powers should be moderated; the second, that care should be taken not to expose the body to chills, by which internal congestions are now more easily developed. Large veins are indications of a little more sleep, a little more warmth. This is scientific palmistry, and suggests a great deal in biometrical calculations.

*WATER GURGLE IN THE DIAGNOSIS OF THE POSITION
OF STRICTURE OF THE ŒSOPHAGUS.*

WE sometimes find it a difficult point of diagnosis in œsophageal stricture, to determine satisfactorily the precise seat of the stricture.

The probang gives us the fact fairly well when the stricture is in the upper third of the tube, but when it is low down there is less certainty. One day, in a case of doubt of this kind, I withdrew the probang, and requested the patient to make a vigorous effort to drink a glass of water whilst I auscultated in the fore and back part of the chest, along the line of the œsophagus. The result was the detection, at once, of the seat of obstruction. At a point very low down, within two inches of the termination of the œsophageal tube in the stomach, a loud gurgling sound, followed by a sharp noise of a passing current through a constricted passage, was distinctly audible. To some extent, this test is useful in distinguishing spasmodic from organic stricture. When the stricture is spasmodic the closure of the tube extends, with more or less of intensity, through the whole of the tube, and the attempt to swallow is attended with no gurgling sound whatever, but, generally, with a great deal of nervous or hysterical movement of the patient altogether, and with expression that it is of no use trying to carry out the effort. But in the organic class of case the patient is anxious to assist in the diagnosis, and will endeavour, in the steadiest manner, to swallow the fluid during auscultation. One patient under my care had anticipated me; he had himself heard the gurgling noise at what he supposed to be the point of stoppage. It turned out that he was quite correct in his observation.

THE SENSE OF SAFETY.

THE sense of safety is a sense which cannot be over-estimated. In actual danger of death it is often a saving cause. Let two men be subjected to a prolonged fast; let one feel that he has nothing at command for sustenance except water, and that there is no reasonable escape from the ordeal; let the other be subjected to the same process, but with the knowledge that he can, at any moment, have any food he may please to call for, and the supremest difference will be shown. The man under the sense of safety will outstrip his less fortunate fellow, one, two, or three times in endurance. To come nearer home, let two persons be suffering from the same disease in the same intensity, and let one be visited by a physician who encourages the sense of fear, the other by one who dispels that sense, and the difference is as marked as night from day. The first physician may have all the knowledge, the other the mere tact that gives confidence, and, in the end, the knowledge will often fail where tact will win. This is not a plea in favour of placing tact before knowledge, for some in physic who are tacticians, pure and simple, may in their very success be the most contemptible of human beings, quacks, bare quacks, in nature, if not in name. So, when we hear the people say too emphatically of a man that they have faith in him, it turns out, too often, that they have no suspicion on what their faith is founded. But the ignorance of a man does not destroy the fact of faith in him as an element of cure, and the man of knowledge ought never to wipe off his slate the memo. that knowledge which gives no sense of safety is like a meal at the mouth of a sepulchre.

TREATMENT OF INGROWING TOE-NAIL.

IF all the minor surgical operations there is not one so rough and painful as the orthodox procedure for the treatment of ingrowing nail of the great toe. The big clippers—for they deserve no other name—commonly used for the process; the forcible introduction of the blade and closure of blade for division of the nail down to the matrix, and, the force employed in pulling up the part that is to be removed, mean an extra expenditure of operation, beyond all necessity. I have long given up this method altogether, substituting for it the following, which answers perfectly. Half an hour before the operation is to be performed the patient is directed to immerse the foot in a bath of tepid water, and gradually add hot water until the whole of the water is heated up to what can just be comfortably borne. Then into the water one drachm of common washing soda is added to each pint of the water in the bath, and the foot remains in the alkaline fluid for the half-hour. At the end of that time, the nail will be found quite soft and flexible. The foot is now to be well dried with a towel, and with a strong short-bladed scalpel the nail is to be neatly scraped, over the crown of it, until it is as thin as thin paper. Next, with the handle of the scalpel, or with a flat director, the part of the nail to be removed is gently raised, as far as possible from its holdings; the nail over the matrix is painted with carbolic colloid; a few breaths of ether spray are played over the nail; a blade of a fine but strong pair of scissors is passed under the nail in the usual way; the division down to the matrix is effected; and, the offending structure is gently evolved and eased out of its place. Every stage of the operation is entirely

painless, there is no suffering from reaction, and a pledget of cotton wool treated with styptic colloid is all the application of dressing that is needed. If carbolic colloid be not at hand, a drop of solution of carbolic acid will answer; and if styptic colloid be wanting, Friar's Balsam—compound tincture of benzoin—containing ten grains of tannin to the fluid ounce, will serve the purpose for dressing the wound. In the course of the past twenty years, I have performed the operation for removal of ingrowing nail, in the manner here described, six times, and in each case with perfect immunity from pain and with complete success.

CARBOLIC COLLOID.

ARBOLIC colloid is styptic colloid containing carbolic acid in the proportion of twenty grains of the acid to an ounce of the colloid. It can be obtained ready for use, but it is easily made when wanted, if styptic colloid and the acid be at hand. It is a most useful compound, being styptic, anæsthetic, and antiseptic. Anhydrous ether dissolves in it readily, and an ethereal solution of it applied to skin or mucous membrane, by means of cotton wool held in the forceps, will cause, by the evaporation of the ether and the local anæsthetic effect of the acid, quite sufficient local anæsthesia for many small and superficial operations, such as snipping off a small hæmorrhoid, or a small pedunculated tumour. It also gives quick relief to an aching tooth if used with wool as a plug, and it is effective for laying open an abscess, painlessly, if the incision has not to be carried through too deep a surface.

*BENJAMIN BELL, F.R.S.E., AND SYSTEMATIC
SURGERY.*

IN the three past numbers of the ASCLEPIAD we have had before us types of the leaders of medical science of the early School of Edinburgh, in the persons of Monro Primus, Cullen, and Black. The first of these represented anatomy; the second, theoretical and practical medicine; the third, chemistry and what may safely be called vital physics. In the present essay I propose to study, through one of the leaders of the same school in its early days, a representative of surgery in the person of Benjamin Bell, one of the surgeons to the Royal Infirmary of Edinburgh and the author of that famous "System of Surgery," which first brought together the art of surgery in broad and yet orderly form, presented the art to the student after the manner of a classical and authoritative work, and "crossing the Border," entered, in its day, into the libraries of the English as well as the Scottish practitioners, ranking as the guide-book of the qualified man, the text-book of the student.

By descent our present master—surgeon, author, and sanitarian; for I shall have to refer to him in all these lights—was a "Covenanter;" not one extreme and active, but latently so, and solidly earnest.

The Bells were a clan in "the old Border times," so a later Benjamin Bell tells us; and George Bell, the great-



Asclepiad No. 28 Vol. VII London, October, 1890.

BENJAMIN BELL

F.R.S.E.

*Autotype, from a photograph,
by Moffat, of a medallion by
Tassie - 1792 - now in the
possession of Dr Joseph Bell.*

BMR.

grandsire of the subject of the present memoir, was both a Covenanter and a rebel; such a rebel, that although he was included in the Act of Indemnity in 1662, he had to pay for his release the good round sum of one thousand pounds in Scottish money.

George Bell had two sons, William, the elder, and Benjamin. William became heir to the paternal estates, including the home farm of Blackethouse on the banks of the Kirtle, in the parish of Middlebie. He did not retain the property very long. He sold it all, probably to cover family liabilities; but his brother Benjamin, who occupied one or more farms under the Duke of Buccleugh, and took to the rearing of stock, succeeded so well that he bought back Blackethouse, an old tower near by, and some adjacent lands called Cushathill.

Benjamin Bell, the rearer of stock, married a Cumberland lassie, Rebecca Graham, of Breckon Hill, and amongst other children of his family had a son whom he named after his own father, George, and who, in his day, became a farmer and breeder of stock at Woodhouselees. This second George was an eccentric character, fond of practical jokes, in carrying out which he sometimes took a personal and prominent part. In the year of the Rebellion, 1745, he married Ann Corrie, a daughter of James Corrie, Esq., of Speddoch, in Dumfriesshire. It was a long and happy union, extending over seventy years, and resulting in a family of fifteen children, the second of whom, and the eldest on the male side, was Benjamin Bell, the man destined to become the leader of surgery for a long time in the metropolis of the North; our surgeon, author, and sanitarian.

The family record states that Mrs. George Bell was a lady of marked individuality. She was calm and placid in temper, and exercised the most beneficial influence over

husband and children. She lived to have her portrait painted by Sir Henry Raeburn when she was nearly ninety years of age. She was of Puritan descent, her great-grandfather having been Alexander Gordon, "the Covenanter prisoner of the Bass," whose faithful wife was Janet Hamilton, daughter of Sir Thomas Hamilton of Preston, one of the heroes of Dunbar and Worcester. She survived her husband, although he lived until the year 1813, and reached the patriarchal age of ninety-one. Mr. Bell was a man of original cast of mind, respected by all his circle in spite of his eccentricities, and noted his country round for introducing turnips and clover into the agriculture of his district.

OUR SURGEON AND PRACTITIONER.

The subject of the present memoir, son of George Bell and Anne Corrie, was born in April 1749, in the town of Dumfries; and, after his preliminary education under Dr. Chapman, rector of the Grammar School, was apprenticed, according to the custom of the day, to Mr. James Hill, a surgeon in Dumfries. He was, in a side way, connected with medicine, for one of his aunts on his father's side was married to Mr. John Mowat, a surgeon in Longtown, a man born in 1660, who lived to 1776, a period of one hundred and sixteen years, who performed the operation of lithotomy after he had completed his hundredth year, and who some years later, on going to visit, professionally, his friend Dr. Graham of Netherby, fell upon the Esk while it was frozen, and broke his thigh bone in the fall, but recovered, and resumed his work as a country surgeon.

After serving his time with Mr. Hill, Benjamin Bell proceeded, in 1766, to Edinburgh, a lad of seventeen, to become in due form a student of the already famous

medical school. Our previous studies relating to medical life in Edinburgh at this period will convey the fact that he commenced his work under the most favourable auspices. *Monro Primus* was still in life, and was now acting as a teacher in the clinical part of surgical art; but his place in anatomy was filled by *Monro Secundus*, Alexander *Monro*, a man who, as I think, was an abler teacher, and who certainly has left better scientific work behind him. *Cullen* was rising into his fame, and *Black* was also making his way. Altogether the start was good for Benjamin Bell, and he made the most of his opportunities. In November 1767 he was a dresser in the surgical wards of the Infirmary, and in May 1769 was taken on trial as surgeon's clerk, to which post he was duly elected by the managers, on the recommendation of the staff, on July 3rd of the same year. On January 1st of the session 1770 and 1771 he was re-elected, resigning the post on April 1st of the last-named year.

Some interesting letters written by Bell to his parents at this period of his life, give us an insight into practice as then carried on. His mother seems to have been a sufferer from headaches, for which, naturally enough, she consulted her firstborn, now surgeon's clerk in the great Infirmary. He sent her for treatment bark and wafers. One of the wafers was to be floated on water until it was rendered soft; then a portion of the bark, in powder, as much as would cover a shilling, was to be put on the wafer; the wafer was to be doubled up, and the whole swallowed. It would go down "without the least taste," and the dose might be repeated four times a day. In the same letter he prescribed for his tiny brother Eben, who was teething. The mother, if she should see an inflamed gum, was to scarify it with one of her nails, and was to insert a pea-issue either in the neck or arm. Mention is

also made in the letter of an outbreak of cattle plague on Captain Ewart's farm in Nithsdale, and it is recommended that if the same calamity should break out at the farm at home the affected animals are to be killed on the spot. "There is no other way," he says, "of curing the disease, it is so quick in its progress; often so quick that the beasts drop down dead the minute they are exposed to the contagion." To which he adds the curious fact, showing that our legislators knew how to stamp out cattle plague over a century ago, "There is an Act of Parliament requiring every person to do so, and at the same time entitling them, from the Government, to every farthing's expense they suffer in that way. And, accordingly, when the disease, about two months ago, broke out in the West Highlands, two hundred cattle were immediately killed, which effectually prevented, with other measures, the contagion spreading further."

With the view of his resignation of the office of surgical clerk before him, Bell began to think of making a tour to London and the Continent in order to improve his professional, and especially his surgical, knowledge. He explained his reasons for this step in a letter to his father, bearing date January 19th, 1771. If he had been entering the world, he observes, as a physician, he would never have thought of going further than where he had been; but for a surgeon Edinburgh comes greatly short of either Paris or London, and for that reason Dr. Monro and others to whom he had spoken upon the subject "approve of the scheme very much." The tour will cost £150, a sum which he thinks will be well spent in the advantages that must accrue. The information is of value to us now as an indication, historically, that Edinburgh was not great as a surgical school in its early days, though very great as a medical; and that the usual continental tour

so much coveted by all ambitious students was not so expensive, after all, as has been supposed. The journey took place, and lasted some months; but unfortunately few details of it have been preserved. One letter, addressed to Cullen from London on March 30th, 1772, deserves, however, a passing notice, as throwing, amongst other things, a glimpse on the brothers Hunter, then in London, and on the tide of their success:—

“Amongst other obligations which I lie under, that of your letter of introduction to Mr. Hunter I consider as none of the most trivial, for by it I have had the pleasure of a most agreeable and, at the same time, the most useful, acquaintance I ever met with; for there is scarce an article, either in physic or surgery, that Mr. Hunter has not something new upon, and there is none more ready of communication than he is. I am also just now attending Dr. Hunter’s anatomical course, which is indeed extremely ingenious and satisfactory; but the Doctor is by no means so free or so ready of access as his brother.”

In the course of this letter there is a notice of the publication of Hulme’s essay on puerperal fever, and of Macbride’s volumes of the practice of physic; but the most curious part of the letter relates to a visit paid on March 26th to the Royal Society, when a paper was read by Dr. Priestley upon the nature and properties of air. “Priestley,” he remarks, “adopts Dr. Macbride’s theory of fixed air and putrefaction, and mentions the effects of such air on putrefaction.” One very bad case of putrid fever was related, in which fixed air was injected into the bowels by the rectum in considerable quantities; the patient seemed to be relieved from the first application, and to be cured by it. In this same paper Priestley also explained his immortal discovery that mephitic air, confined with

growing plants, is not only thereby entirely changed and again rendered pure, but the growth of such plants is likewise considerably quickened; from which he concluded that the bad effects of the breathing of animals upon the atmosphere are counteracted by the plants which are everywhere found upon the earth, and that such air is even, in some degree, necessary for the proper production and life of plants. In the same letter, Bell describes the discovery of "a new acid," by the famous chemist Scheele of Stockholm—Scheele's hydrocyanic acid, as we now recognise it.

By this time, in the course of his life, Benjamin Bell had become, in a curious way, a man of substance, and, in point of fact, the landlord of his own father's farm, his grandfather, after whom he was christian-named, having left him his property upon the demise of his Uncle Thomas, who had a life interest in it. Uncle Thomas died some time before 1772, and so the property fell into his hands, his own father having been passed over, perhaps because he had twice failed in business in early life, perhaps because the older man had a special affection or admiration for his grandson. The position was a painful one for the young surgeon; but he carried out the duties of possession with remarkable tact and filial respect, even under circumstances particularly disadvantageous, as the correspondence on business affairs between father and son demonstrates.

At the age of twenty-four Bell returned to Edinburgh, and commenced practice in a very simple way. He took up his quarters with his sister Rebecca, and is not ashamed to say that he had a "shop," which he purchased of a man about to decline "business," and which he bought over, "drawers, counters, mortars," etc., and set up "in a room above stairs." His sister still calls him Ben, and begs her

mother to send them butter, and another quilt for Ben's bed, because the one she bought is too large and it would be "a pity to cut it." The start was simple enough, but it went on for a time excellently. The young surgeon, with a shop, and a quilt too large for his bed, was elected in the first year of his practice, namely, on August 3rd of the year 1772, Surgeon to the Royal Infirmary;—an office he held for twenty-nine years—and practice came steadily in.

After being in practice for about three years Bell married Grizel Hamilton, the daughter of Professor Robert Hamilton, Professor of Theology in the University, and twice Moderator of the General Assembly. His marriage with Grizel Hamilton was one of the unlooked-for events of his tour on the Continent. During that time he had as a companion the brother of his future wife, James Hamilton, afterwards the author of the well-known book on the administration of purgative medicines. The two young men were fellow-boarders in Paris, in the house of the distinguished Baron Portal, whose works to this day are a part of every good medical library, and, who had already published his two volumes of the "*Précis de Chirurgie Pratique*." Companionship under such favourable circumstances brought about a warm friendship and, ultimately, a relationship, by marriage, of a near and close kind, between the two young men; a relationship favourable to them both, and happy to the end.

Life is chequered. In the first period of his married and promising career Bell met with a physical accident that disabled him for two years. He sustained a fall from his horse, which prevented him following his professional work, and filled his mind with fears that he would never recover sufficiently to resume that work. With much resolution and resignation he had himself

removed from Edinburgh to Liberton, three miles south of the city, where he took a farm on lease, and having a natural liking for agricultural pursuits, determined to settle there if the worst came to the worst. Fortunately his gloomy expectations or fears were not realized. A story that his mind had become affected was untrue, and under rest of body he made a sound recovery. The repose was not lost, for during the two years of it which he had to bear, he made preliminary provision for the works of authorship he had in store after practice had ripened his experience. Fortunately, also, the taking of the farm at Liberton was not without its good, for the farm continued to be his summer residence for many long years, and was to him a blissful place of rest in health and in strength, as it had been in sickness and debility.

On resuming his professional duties in Edinburgh Bell resided in Assembly Close, his sister Rebecca still remaining with him. Before he left practice to retire to Liberton he was making about £400 a year; and now, although he was at first enfeebled and unable to climb six-storied houses to visit the sick, he soon made new headway, and "got fixed at last in a very good house, well aired and lighted, with an easy access of one story from Niddry's Wynd, and an entry from Kinloch's Close, without any stairs." This was in September 1777. Next year he required two horses from his father for business purposes, horses that would run together, since he had given up horse-riding because he had no time for it,—good signs of increasing labour in the practical line. He was also on the look-out for the appointment of Surgeon to Watson's Hospital, to which office there was a little emolument; an office which he obtained in 1778, and held for the remainder of his life, and which passed through three succeeding generations of the Bell family.

In 1777 the rising surgeon was blessed by the birth of a son, christened, by his grandfather,—Professor Hamilton,—on the day of his birth, February 2nd, George. The news is conveyed in a letter to Grand-mamma Bell, with many details of family interest. The fact is told that the happy mother was attended by a midwife, and was doing so well that this important functionary was dismissed soon after the great event was over, and that Mrs. Bell had her mother with her, so that she could “be at no loss” for attendance. The child thus born was a child to be proud of. He became, in due course, a member of the fraternity of physic; distinguished himself as a student of the London Hospital; went back to Edinburgh to join his distinguished father in practice; and, like his father, left behind him as honoured if not as famous a name.

A letter addressed by Bell to his father in July 1778 indicates that he had been in partnership for a time with a Mr. Gibson, a man of good repute, and that this co-partnership was about to be dissolved. In the autumn of this same year he had a daughter born to him, who received the name of Jane. Another letter to his father, dated February 4th, 1779, tells a story of wild religious excitement in Edinburgh against the Roman Catholic Bill. The well-wishers of the Protestant cause were invited to meet on a Wednesday evening to pull down a monument of superstition, an elegant house lately built as a Catholic chapel at the foot of one of the northern closes. The day named was a feint, for the people met on the previous Tuesday evening; and, although the town guard and four hundred of the South Fencibles were turned out in an instant, and long before any mischief was done, yet even in the face of such a number of armed men, with the magistrates of the place at their head, the furniture of

the chapel was first of all destroyed, after which combustibles were got at, and the building, in a few hours, was reduced to ashes. Several individuals of the Romish faith were attacked, and next day, in the open sunshine, the houses of different people of that persuasion were ransacked. One woman, Mrs. McDonald, was used so barbarously that she and her child, of which she had been delivered only a few days, died on the same day. Even Protestants who expressed themselves in favour of granting religious liberty to Roman Catholics were doomed to suffer. Principal Robertson's windows were entirely demolished, and he and Mr. Crosby—the original of Sir Walter Scott's Mr. Pleydell in "Guy Mannering"—and Sir John Dalrymple, were marked out for revenge. Three troops of dragoons, brought into the town, finally quenched the outbreak, and restored order.

For many years after this date Mr. Bell's practice continued to increase, and so trusted did he become, it was a saying in Edinburgh that no one would willingly die without being visited by him. His practice extended beyond the mere domain of surgery and surgical art; yet, above all, he was the operating surgeon, careful and conservative in his selection of cases for operation, skilful in diagnosis, firm, precise, decisive, and ready in the course of all his operative procedures. How much he was esteemed is conveyed in a communication written by the late Mr. James Wardrop, a London surgeon who died not a great many years ago, and to whom I was once introduced by Sir William Fergusson. Wardrop looked on Bell as one of the best and shrewdest observers he had ever known. He knew Bell in his early life, owing to the fact that his uncle, Dr. Wardrop of Edinburgh, was for a time one of the firm of Bell, Wardrop, and Russell. Speaking of Bell as he remembered him in the year

1796 and later on, he remarks of him: "His manner was devoid of every kind of ostentation. He was of a kindly disposition, and in stating his opinion made use of very plain and accurate language. He had an impressive mode of expressing himself, giving great assurance and confidence to the sick. In all the excitement of surgical operations he displayed the greatest composure. He was a successful operator, and during many years was more employed than any other surgeon in Scotland." James Wardrop was, for a time, admitted into a junior copartnership with the firm, and knew all the members of it in private as well as in professional life. The happy privilege led him to form as high an opinion of Mr. Bell in one position as in the other, with a particular admiration of his kindness to the junior members of the profession. In his eyes Bell compared in all respects, favourably, to other teachers and masters, save and except *Monro Secundus*, who seems to have won golden admiration from contemporaries of all classes, as I hope to show at some future date. Another good quality related of Benjamin Bell was that, although admirably stored with information, he always, in conversation, appeared to be the learner, even when he was communicating information, in this respect carrying out the recommendation of Alexander Pope:—

"Men should be taught as though you taught them not,
And things unknown be told as things forgot."

In the most pressing parts of his work he was never in a hurry.

A little incident, which may be most appropriately introduced here, connects Benjamin Bell with the present generation and with an Edinburgh representative man of light and leading in our profession. Benjamin Bell had a favourite brother named Joseph, whom he hoped one day

to have with him as a partner in his practice. It was not to be. Joseph Bell went abroad, and died about the year 1786. In the same year Benjamin Bell had born to him his youngest son, who at first received the name of Thomas. On the news arriving of the death of brother Joseph the name of this child was changed to that of Joseph. Joseph Bell, after being a pupil of Blizard, Astley Cooper, and Abernethy, became, in his turn, a surgeon in Edinburgh, almost as distinguished as his father. Joseph Bell died in 1848 of fever contracted in the discharge of his professional duties, leaving amongst other children Benjamin Bell, author of the Life of his famous namesake, a man much beloved, and the father of the present Dr. Joseph Bell of Melville Crescent, Edinburgh, of whom, if I could but speak as I would, I should indeed be happy, although no word of mine could add to the appreciation in which he is held as one of whom any of his forefathers might truly, if he could have forecast, have declared, "*Omnes omnia bona dicere, et laudare fortunas meas, qui gnatum haberem tali ingenio præditum.*"

THE SYSTEM OF SURGERY.

With the exception of two or three essays on matters extraneous to medicine, the reputation of Benjamin Bell, as an author, rests on his six volumes entitled "A System of Surgery." These volumes were published one after another, each volume being fairly complete in itself, although in one, as appears in the edition before me, the subject at the close of the third volume—Diseases of the Eyes—extends into the fourth. The plan of the work was contemplated by its author in his early life, at the time, as we have already seen, when he met with the accident which held him so long a prisoner at Liberton. In this period he sketched out the work, taking, as I

infer from the preface of the first volume, the "System of Surgery" published by Heister in 1739,—a work which he considered was the most systematic up to his time,—for his model.

Volume one, dedicated to Dr. Alexander Monro, bears date November 1792. The design is given by the author in the following terms :—"To exhibit a view of the art of surgery as it is at present practised by the most expert surgeons in Europe, as far at least as my own observation, in the course of attending different hospitals, joined to the advantages of reading and correspondence, have enabled me so to do."

Although called a "System of Surgery," there is admittedly no kind of classification or methodized arrangement of the subjects under consideration; the reason for this absence of arrangement being that disorders of every kind requiring the assistance of the operative part of surgery are perfectly local, and unconnected with one another by means of symptoms common to each other; and because it seldom happens that there is much similarity in the means necessary for the removal of such diseases. "The parade of classification under such circumstances, although it may serve to display the fancies of an author, can have no effect either in rendering the study of surgery more easy, or the practice of it more attainable."

The subject-matter of the first volume is on sutures; ligature of arteries and other means employed, by art, for putting a stop to hæmorrhages; blood-letting, to which subject eighty-nine pages are devoted; aneurisms, hernias, hydrocele, hæmatocele, varicocele, sarcocele or scirrhus testicle, and, lastly, diseases of the penis.

The second volume, published in 1784, with a dedication "To Alexander Wood, Esq., Surgeon in Edinburgh," is on stone; incontinence of urine; suppression of urine;

obstructions in the urethra; fistula in perineo; hæmorrhoids; condylomatous excrescences and similar affections of the anus; prolapsus ani; imperforate anus; fistula in ano; paracentesis of the abdomen; paracentesis of the thorax; bronchotomy; œsophagotomy; and amputation of cancerous mammæ. In the introduction to this volume it is explained that the execution of it was delayed by the circumstance that the author had been engaged in publishing a third edition of a treatise on ulcers, inflammation, and white swelling; a treatise which must have commanded a large amount of attention in order to call for a third edition.

The third volume, dedicated to the managers of the Royal Infirmary of Edinburgh, commences with the study of affections of the brain from violence, a subject which extends through two hundred and forty-three pages in six sections. It is followed by a chapter of eighteen sections on Disease of the Eyes, the volume closing with this chapter on the four hundred and sixty-ninth page. The different affections of the eyeball demanding surgical treatment are discussed at great length, and a comparative view is tendered of the respective advantages and disadvantages of the operation of couching and of extraction of cataract.

Volume four, without a dedication, was published in 1787. It opens with a new section on Diseases of the Eyes. It had not been the intention of the author to treat again on this subject, but a foreign oculist, M. Jean François Pellier, having appeared in this country, where he quickly acquired great reputation, Mr. Bell considered it a necessary addition to this part of his work to extend the chapter on eye diseases so as to include such parts of M. Pellier's practice as appeared to be of importance. Pellier divided cataracts into three varieties: (a) The curable, known by the pupil retaining its natural

power of contracting or dilating in full perfection, the patient being at the same time able to distinguish the light of a candle or of any other luminous body, and even bright colours, such as red and green; (b) The mixed or doubtful cataract, "attended with feeble contraction and dilatation of the pupil, where the patient can scarcely distinguish light from darkness, along with an opacity of the crystalline." This state is supposed to be attended with an affection of the retina or of some other part of the eyeball; (c) The false or incurable cataract, where, with an opaque state of the lens, there is a diseased state of the pupil, which remains always immovable to whatever degree of light it may be exposed, the patient at the same time being unable to distinguish between the most brilliant light and perfect darkness. These, with many other observations of Pellier, are introduced and accompanied by methods of operating, which are held to present various novelties of detail and management. This additional chapter is followed by chapters on diseases of the nose and fauces; diseases of the lips; diseases of the mouth; diseases of the ear, and operations practised on them; wry neck; diseases of the nipples; issues; and inoculating for the small pox.

Volume five, also without a dedication, appeared in 1788, and is devoted to wounds of different parts of the body; followed by chapters on burns and tumours. It is curious to observe, in the chapter on tumours, how much more widely the term "tumour" was applied a century ago to what it is now. Thus erysipelas is set down as an acute inflammatory tumour; lumbar abscess is ranked as a tumour; and even sprains and contusions come under the same head. In short, the one idea of "swelling" furnishes the idea of the word "tumour," whatever may be its cause or its nature.

Volume six, the last of the series, was published in May 1788, without a dedication, but with a preface. The author now declares the "System of Surgery" completed, as he had undertaken to write it originally. It had, he tells us, met with a reception more favourable than he had expected, and more flattering than it seemed to him to merit. He will, however, so he promises, insert in every new edition "whatever improvement future experience may add to the stock of chirurgical knowledge." In this last volume he discusses: fractures of various kinds; luxations; distorted limbs; distortions of the spine; amputations; removal of the ends of bones in diseases of the joints; preventing or diminishing pain in chirurgical operations; midwifery, including the Cæsarian operation; division of the symphysis pubis; the opening of dead bodies; embalming; and bandages.

The "System of Surgery" was published by Charles Elliot in Edinburgh, and on the title page bears his initials, C.E. In London it was published by Elliot and Kay, 332, Strand, and by G. G. J. & J. Robinson. The price of each volume, in boards, was six shillings and sixpence. The work throughout is well illustrated with copper plates.

THE SCOPE AND CHARACTER OF THE WORK.

The late Sir Benjamin Brodie, in his Autobiography, was rather severe in his criticism on the "System of Surgery." He describes it as an "unreadable production." The criticism was not just. The work is not, it must be admitted, marked by elegance of style,—the author knew that himself; but to say that it is not readable is quite away from the fact. It is a work characterised by perfect clearness; not a description, not a direction in it, that cannot easily be followed. More than that, it *was* followed,

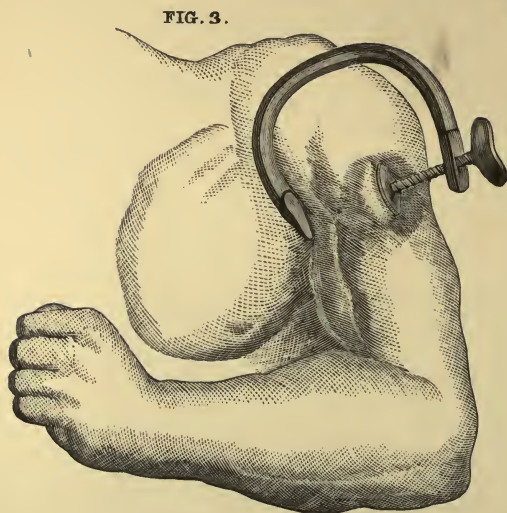
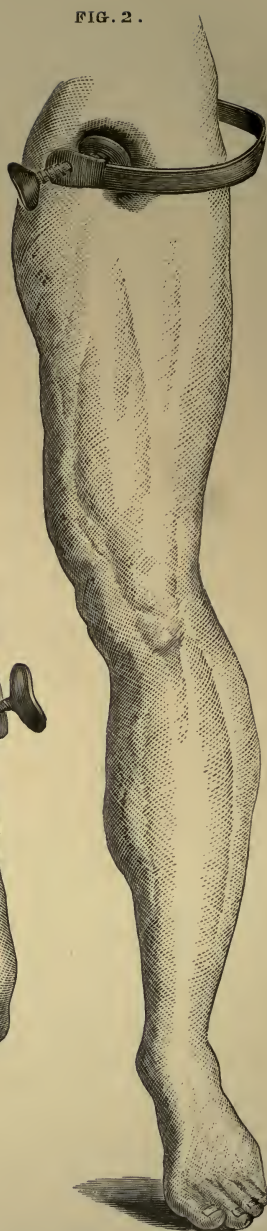
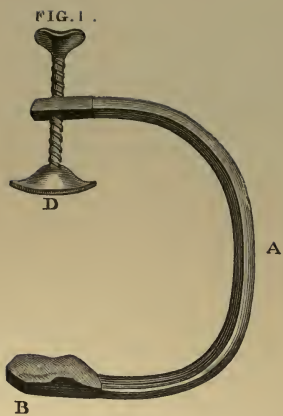
which is the best proof of all of its clearness; for men of the past, slow though they might be, were not so stupid as to purchase seven editions of a work at six shillings and sixpence a volume, if it had not been very clear and instructive. In point of fact, it was a good, sound, practical book for its day and generation, and, without serious paradox, was the more systematic because it was not systematised. It followed the system of nature without the slightest attempt to classify artificially, and it asserted itself so little that without considerable study of books preceding it it cannot be said how far it was or was not original. We may suppose that it did not contain any such striking discovery as would affect the critical mind of Brodie; and we may also suppose that much it once taught had in Brodie's time become absorbed into such common general knowledge as to seem commonplace to him. But Bell was a man who compared what he read with what he observed, and so blended the two that the two labours became, in his mind, one. My estimate of him is that he was not enthusiastic in matters surgical or medical, but was marvellously conscientious, and as marvellously industrious. If he had had his own way, or if, by fate, he had found his own way, the way which would have suited him best and in which his genius would have moved in its true sphere, he would have taken place amongst the great if not the greatest of his compeers. But that sphere was not the sphere of medicine or surgery. He was thrown into the medical sphere, and, like a plain, honest, upright man, he laboured in it to the highest pitch of his ability in that direction of labour. His true sphere was that of philosophy and political economy. He belonged, naturally, to the school of his friend Adam Smith, rather than of Monro, Cullen, or Black.

With this impression on the mind, it is fair, at the same

time, to state that in surgery Bell was often far-seeing, if not original. In proof of this let three instances be submitted.

(1) When he is treating of paracentesis abdominis, he treats also on the value of tapping the intestines when they are largely distended with gas. For this operation he suggests the use of a small trocar, with which to make the puncture. The effect produced on the breathing by "tympanites" is, he says, nearly the same as that from accumulation of water in the abdominal cavity; but the swelling is much more tense, and gives to the touch the sensation of a bladder distended with air. In such cases, when the oppression is very considerable, and danger is imminent, no doubt should be entertained of the propriety of discharging the gas as soon as it is found to be productive of great distress. And this may easily be done, in the manner directed for ascites, if care be taken to use a trocar of the smallest size, and to employ pressure in the same guarded manner as when the swelling has been produced by the accumulation of water.

(2) In speaking of amputation he is most particular in insisting on the practice of saving as much skin as will thoroughly cover the wounded surface and help to exclude the air from the interior of the wound. Belloste had already proclaimed, in the most explicit form, the success of healing wounds by the exclusion of air; and Bell seems to have been alive to the vital importance of the same practice. "Save skin," was his watchword, "and in all amputations give abundance of flap, for by this the surgeon will do most towards securing the good result of healing by the first intention." To secure this object, in amputating in the lower third of the thigh Bell cut a long anterior flap, almost precisely after the operation



MR. JAMES MOORE'S METHOD OF PRODUCING LOCAL ANÆSTHESIA BY
PRESSURE. 1772.

FIG. 1.—*The Compressing Instrument.*

FIG. 2.—*Instrument with Compress of Leather on Sciatic Nerve.*

FIG. 3.—*Compress with Screw on Crural Nerve.*

since performed, with so much recognition and proper favour, by the late Mr. Teale at Leeds.

(3) It is due to Benjamin Bell to say that he was one of the first in this later era to study the introduction of methods for the prevention or diminution of pain in surgical operations. "To be able," he remarks, "to alleviate the misery of those who are about to submit to dangerous operations must afford the highest gratification to every practitioner; and, as pain is the most dreadful part of every operation, it necessarily demands our most serious attention." He then proceeds to state that pain may be lessened in different ways—"by diminishing the sensibility of the system; by compressing the nerves which supply the parts upon which the operation is to be performed; and by narcotics, of which nothing answers with such certainty and effect as opium." He had an objection to the administration of narcotics in such large doses as to prevent or diminish pain, because they produce sickness and vomiting; but towards the local method of causing insensibility, by compression of the nerves, he was much inclined. He had observed that patients undergoing amputation frequently desire the tourniquet to be firmly screwed, and he supplies a drawing, of which I reproduce a copy on the opposite leaf, of an instrument proposed by Mr. James Moore of London for compressing the principal nerves so completely as to render the parts beneath altogether insensible. All that this instrument seemed to require to render it perfect was, he thought, the power of compressing the nerves of a limb without affecting the veins; for as it is found that the nerves must be compressed for a considerable time, at least an hour, before the parts are rendered altogether insensible, the veins could not be compressed for such a length of time but with the risk of bursting. To

meet this difficulty Moore proposed opening a vein; but Bell suggested a better plan, namely, compression of the artery, so as not to permit the veins to be distended.

MISCELLANEOUS ESSAYS.

There are many other medical and surgical essays of Bell on which I might profitably rest. In the first volume of the Medical Commentaries, edited so ably by Dr. Andrew Wood, he reports a case of obstinate epilepsy considerably relieved by the use of flowers of zinc. This was in 1773; four years later he published his treatise on "The Theory and Management of Ulcers, and on White Swellings, accompanied with a Dissertation on the Chirurgical Treatment of Inflammation," the work being dedicated to Mr. Percival Pott. In this work he insisted on the practice of opening large abscesses without the admission of air, giving the credit of the suggestion and of the practice to Mr. James Rae, who introduced the method with the most marked success in the Royal Infirmary of Edinburgh in the year 1747. In respect to the treatment of malignant tumours, he taught the good practice of early operations for their removal by the knife, on the ground that at their commencement they may be considered as local affections. Two works also came from his industrious pen, on the subjects of virulent Gonorrhœa and Lues Venerea or Syphilis. Here he enforced the view that the poisons of these diseases are different in character, and that the diseases themselves are specifically distinct; a view not only original and largely independent in his time, but opposed to the opinions of no less an authority than John Hunter. He wrote also very correctly and clearly on the subject of strictures of the urethra, dividing such strictures into spasmodic and organic, and combating and correcting successfully a

then current error, that strictures are due to venereal ulceration in the urethra, and that the administration of mercury until the body is brought under the mercurial influence is essential for their cure. In his practice he instituted mechanical means for the cure of stricture by dilatation with bougies, with, if necessary, counter-irritation by means of a blister to the perinæum; methods we in this day have not much improved upon. In speaking of syphilis, he opposed Mr. Hunter's theory, that no two diseases can exist in the constitution at the same time, and adduced in proof of his argument the co-existence of syphilis both with scrofula and scurvy. On the whole, I think we may give to Benjamin Bell the credit of defining for the first time, demonstrably, the distinction between gonorrhœa and syphilis; of having shown that gonorrhœa could be treated successfully without the use of mercury; and of having pointed out that while gonorrhœa leaves no constitutional taint after recovery from it, syphilis may leave the most decisive constitutional taint transmissible from one generation to another. He was skilful, too, in the diagnosis of chronic syphilitic affections simulating other local affections. Called to operate on a gentleman supposed to be suffering from malignant disease of the testicle, he detected the specific rash on the body, withheld the use of the knife, and treated with mercury; a remedy in which he had so much confidence that he said of it, "We are more certain of curing this disease, syphilis, with mercury than of removing any other disease by any remedy whatever." He employed mercury by fumigation when it was desirable to affect both the seat of the local disease and the system with rapidity and promptitude; but his favourite method of administration was by inunction of a drachm and a half to two drachms of the ointment twice per day. If there were any local

symptoms he had the inunction applied to the locally affected part; if there were none, he had it applied at any point of the cutaneous surface, changing the spot each time.

In the treatment of ulcers of the lower extremities Bell used the spiral bandage as the best mode of treatment; he tied hæmorrhoids with the double ligature; and, as his illustrations indicate, he used the best instruments that could be produced in his day for all his operative work. In brief, he was a sound surgeon, and the more his works are studied the more they impress the mind. There is enough in them to make one good volume of surgery acceptable and even useful to the wants of the present hour.

OUR SANITARIAN AND ECONOMIST.

Hitherto we have studied Benjamin Bell as the surgeon and busy practitioner of the healing art. Before we leave him we are bound to look upon him in another light, namely, as an advanced sanitarian and economist. In 1783 he commenced to publish essays of the utmost importance on national topics, as on the "Taxation of Income," "The National Debt," "The Funds and the Sale of the Land Tax," "The Improvement of Agriculture," and "The Scarcity of Provisions and Dearth." These essays, collected in 1802, ought all to be republished, for they are the historic bases of many of the greatest social reforms of our time. He first proposed in these essays, in a memorial sent to the Government in 1783, the taking of the census, together with a survey of the whole kingdom; the quantity of corn sown annually and the number of horses, with other information useful to the political economist. He made a vigorous protest against the window tax, explaining that the imposition of this insane assessment was fraught

with the utmost danger to the health of the people ; and he offered many suggestions for the improvement of agriculture, including facts in favour of the early sowing of wheat, that is to say, in the month of September, a plan he had adopted with advantage in the year 1778.

But the grandest political economic idea of Bell had relation to the corn laws. He was the first pronounced Corn Law reformer. To the poet Ebenezer Elliott, to Cobden, Bright, and Sir Robert Peel we assign the credit of the powerful advocacy of these beneficent laws ; but which of them ever spoke more wisely on the folly of the old statutes and the value of a projected new law than this pioneer of a new era of food supply for the people ? “ Let every statute be repealed that has hitherto been continued against dealers in corn and other provisions ; let all who deal in them be protected, and permitted to purchase and sell in all situations and circumstances, and whenever they may think proper. When this shall happen—and the period is not probably distant—it will soon become matter of surprise and wonder that any of these pernicious statutes were continued till the nineteenth century. They will be considered as the more remarkable from being kept in force by the people of Britain, who have long been convinced of the utility of freedom of trade in every other article, and hereafter will be spoken of in the same manner with all statutes that authorize persecution, where full liberty should be given.”

FINALE.

I must not linger longer over the work of this wise and far-seeing surgeon. He gained universal respect ; he won the honours of the Fellowship of the Royal Society of Edinburgh and of the College of Surgeons of Ireland as well as Edinburgh, and maybe other honours fell to him

of which there is no record. His love for agricultural pursuits never left him, and he fostered them whilst still in practice. He had an estate of five hundred acres near Melrose Abbey, and he bought the greater part of a suburb of Edinburgh called Newington, where he had a house, in which he lived during the latter part of his life. He seems never to have completely recovered from the accident he sustained in the commencement of his career; and as his fame increased he felt prematurely the labour of travel on professional errands. Long before he was fifty years of age he began to fail in strength, and, accompanied by his wife and one of his sons, he commenced to visit, for long periods, the southern coast of England. In 1804 he was in Edinburgh doing a little professional work, and he held on until 1806, when he proposed a journey to Cheltenham and Bath, which was never accomplished, although the carriage came to the door to bear him away. His weakness increased, with inability to take food. "I was praying, Dr. Gregory," he replied to a question put to him by Dr. James Gregory, who heard him talking as he entered his room, "I was praying that you might all be forgiven for pressing a poor man to eat when he is totally unable." He died at Newington House on April 5th, 1806, in his fifty-seventh year.

To the practice of the Edinburgh School of Medicine Benjamin Bell added firmness and solidity. The father of its Surgical School, he stands forth a typical figure in a professional sense. The sage and prophet of some of the most remarkable of the political reforms that have occurred between his time and our own, he stands forth as one whom all classes of the nation may esteem; a man of lasting name and fame.

REPORTS, PHYSIOLOGICAL AND THERAPEUTICAL, MADE TO THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE FROM 1863 TO 1872.

PART IV. AN ANALYSIS OF PHENOMENA CONNECTED WITH THE ACTION OF CERTAIN OF THE METHYL AND ETHYL COMPOUNDS.

FROM REPORT READ IN SUB-SECTION—ANATOMY AND PHYSIOLOGY—DEPARTMENT D., BIOLOGY. ON MONDAY AUGUST 24th, 1868, AT NORWICH.

[PROFESSOR W. H. FLOWER, F.R.S., IN THE CHAIR.]

Continued from page 290 of ASCLEPIAD, No. 27, 3rd Quarter, 1890.

HERE are certain of the compounds of the methyl and ethyl series which possess the common property of producing sleep and insensibility in living organisms. This general fact seems at first sight to link them all together ; and in regard to the one fact they are closely united. But there are, notwithstanding, points of difference of the most important character. For example, they all sometimes kill during their administration, but they are not all equally powerful to kill. This alone is sufficient to distinguish them ; and as from them we derive those agencies by which some hundred thousands of our kindred are each year relieved of pain with some risk of life, I have thought it of moment to study the differences of phenomena presented by different agents, so that we may be able to approach to a knowledge of first principles, and seize all the real good, with avoidance

of what at this moment appears an inevitable degree of evil.

INFLUENCE ON THE CIRCULATION AND RESPIRATION.

I have studied as a special point the influence of certain of the ethyls and methyls, with a view to determine the all-important subject of their relative action upon the respiration and the circulation. On this point there has been long controversy,—the most accepted rule being that chloroform always kills by paralyzing the heart, while ether is free from that danger. It must be confessed that at first sight this view seems to be in accordance with the general facts of observation; but a long and careful watching, in which experiment succeeds experiment, leads in time to this certain truth, that chloroform does not always kill by paralyzing the heart, but that under chloroform, as under ether, the heart will continue to beat after the respiration has ceased. The rule, therefore, is not general; the question is how far it is at all true.

To test this, I proceeded first to see whether the heart could be sustained in its action while respiration, with an atmosphere containing a narcotizing dose of chloroform, was artificially supplied. I found in this experiment no difficulty whatever, and no remaining doubt after it. A T-shaped tube having been inserted into the trachea of a strong animal (a dog) narcotized with vapour of chloroform, the animal was made to breathe air charged with the vapour, in steadily increasing doses, until some change occurred. During this time, in the deadest silence from all other motions, the sounds of the heart were listened to, with the double stethoscope, by myself, while an assistant watched for the cessation of respiration. "The respiration stops," was the report; but I could still hear, not only the motion of the heart, but both the sounds

in due order of time. Then the first sound began to fail, and soon the second. At this moment, on a signal from me, artificial respiration, with the same chloroform atmosphere in which the animal had gone to sleep, was set up by means of double-acting bellows; and at once, as the air filled the lung, the blood made its way again through the heart, the motion of the heart returned, the sounds returned, and the general signs of life returned. Once more the experiment was tried, and once again with the same result; and so decided was the experiment that, on giving it fresh air artificially, the animal recovered, in the end, as from simple sleep.

In a further instance I repeated the famous experiment first performed by Hook, with the difference that the animal throughout was held insensible by chloroform. In this case, after insensibility was complete, and respiration, which for some minutes had been carried on through the tracheal tube, had ceased, the heart was directly exposed to view, together with the lungs. The heart was pulsating vigorously on both sides, the lungs generally were blanched, from the blood ceasing to make its way by the pulmonary circuit: but artificial respiration of the chloroform atmosphere was re-established; and as the lungs filled with the air the right side of the heart was set at liberty, the blood passed over the pulmonary circuit, and on withdrawing the chloroform, and for a brief period supplying the air pure, the general signs of life began to reappear. They were, of course, suppressed by repeating the chloroform; but before the heart was finally stopped in its action, the fact of being able to call it again into play by reinducing respiration was several times demonstrated.

Taking an important practical hint from Dr. M'Intosh, who read an admirable paper at Dundee before this

Section, I followed up my research by watching the circulation and respiration in young transparent trout while they were being subjected to the action of methyl and ethyl compounds. I was so fortunate at the close of last year—1867—as to be able to obtain young trout in quantity, and so secure a very good inquiry. I hoped at first to be able to see the motion of the heart on a screen, by placing the animals in a trough in the oxyhydrogen lantern; and I have to thank Mr. Pepper and the Managers of the Royal Polytechnic Institution heartily for placing their very perfect optical chamber at my service. I could not, however, carry out in this way all I desired; for when the figure of the animal was projected on the screen there were no visible movements except those of respiration. I therefore turned to the microscope, and found here all I could require. To carry out my plan properly, Dr. Sedgwick was so good as to devise for me a little trough, in which the trout could be placed for observation, and through which water charged with the required compound could be passed in a steady stream. This plan prevented the accumulation of carbonic acid; and when no foreign substance was introduced into the current, the circulation and respiration of the fish could be observed for long periods naturally.

Thus provided, three substances from one series were made to act upon the trout, viz. chloroform, ether, and chloride of methyl. The observations were most interesting, but to give them in detail would be too long; I shall therefore only offer the results in an abstract, which Dr. Sedgwick, who had the observations daily before him for several weeks, has been good enough to draw out for me.

When chloroform is added to the water in which the young trout is living, in amount sufficient to destroy life in

half a minute, the heart ceases acting at once, and stops in diastole, full of blood. The gill and blood respiratory routine continue some little time afterwards. No contraction of extreme vessels is noticeable for some time after death. If the chloroform be added in smaller quantities, the first effect is violent struggling, which by degrees subsides. The respiration is at first much quickened, then becomes slower, and finally ceases—that is, so far as the gill, fin, and jaw motion is concerned. The heart at first beats quickly, then slowly, after a time intermits, and then totally ceases to act. No contractions of minute vessels could be observed under a power of 600 diameters. Not one of these fishes recovered from the narcotism of chloroform after the respiratory movements had ceased. With chloride-of-methyl gas in water, on the other hand, and also with ethylic ether, recovery was observed after all external respiratory movements had ceased, even for fifteen minutes, the heart beating all the time, but very slowly, the contractions reduced from 246 to 15 in a minute. The phenomena observable in these animals when narcotized with chloride of methyl is in other respects like those which occur under the use of chloroform, the notable points being the cessation of respiratory movements before the circulatory, and the absence of any definite contraction of the small vessels when the agents are administered in quantities not sufficient to produce immediate death.

These experiments prove, I think, beyond cavil, that the chlorides of the methyl series do not of necessity destroy life by their direct action upon the heart. Indeed they indicate rather that when the heart itself is sound, and the chloride is let into the system by the process of inhalation, the heart is not primarily interfered with. It was therefore all-important to learn whether the heart

could be primarily arrested by making it the primary recipient of the narcotic.

To test this, a large animal—a rabbit—was selected, and twenty minims of chloroform were slowly instilled, by one of Mr. Hunter's syringes for subcutaneous injection, into a large vein in the ear. Within a few seconds (indeed as soon as the last minim was injected) the animal was convulsed and dead. As quickly as the operation, with every appliance at hand, could be performed, the thorax was laid open, and the lungs and heart exposed. The heart on both sides was found firmly contracted on firmly clotted blood which filled its cavities. The lungs were intensely congested with blood, also firmly coagulated, so that they cut like substance of liver; the brain was quite natural. Excited by the galvanic current, the muscles of respiration and the voluntary muscles responded well for an hour, but the heart could be roused to no sign of motion.

Here, then, we had a proof that chloroform carried directly to the heart may stop the heart primarily. In this sense, however, it does not act by paralyzing, in the ordinary sense of that word, but by exciting a vigorous contraction, prolonged to the death.

By injecting chloroform through the aorta over the whole body of an animal just dead, I saw the same sudden and permanent contraction universally developed in the muscular system.

The conclusions I have up to this time arrived at in respect to the action of chloroform and all the other chlorides of the methyl and ethyl series, are:—1. That when these are administered by inhalation to healthy bodies, their first action, as they diffuse through the different organs, is exerted directly on the muscles, which they call into powerful contraction. 2. That this action,

extending further to the arterial muscular system, suspends the influx of blood, generally leading, in return, to relaxation of the common muscles, and to arrest of cerebral function. 3. That the heart, if its coronary canals and its walls be good, lives through the catastrophe better than any other structure, and, in short, is the organ on whose power the ultimate recovery rests. 4. That death from chloroform in the healthy animal is due to failing respiratory power, and, above all, to contraction of the pulmonary artery, by which the current of blood from the right to the left side is prevented.

This view, in respect to animals, associates all the phenomena in death from chloroform; it accounts for the stage of muscular excitement, the cessation of the beat of the pulse while the heart is still beating, the continuance of the action of the heart when the respiration has stopped, and the white and bloodless condition in which the lungs are invariably discovered when death is complete.

This same view tallies moreover with all the known facts respecting sudden death under the influence of the chlorides in cases where the heart is not healthy, where, unable to live through the preliminary excitement into which, with the other muscles, it is thrown, it ceases action altogether—ceases for the same reason that it ceased in the animal when the narcotic was thrown direct into it, I mean because it is overcome by the excitation to which it is subjected. A little want of elasticity in the coronary arteries, an undue tendency to contraction in them, a deficiency of muscular substance, an interstitial deposit in the muscle,—any one of these conditions is amply sufficient to account for death from the heart under the influence of the exciting chlorides.

Under the action of the oxides of methyl and ethyl the

living body avoids these dangers to a degree as yet little understood. These from the first produce very little excitability of the muscular mechanism; and when they ultimately kill, the act is not by contraction of vessels, but by slow asphyxia, the gradual extinction of oxidation from the exclusion of air.

Hence, after death by ether, the lungs are not found blanched, but congested, the heart still active, the muscles flaccid, and the blood in all the tissues dark in colour.

INFLUENCE ON THE ANIMAL TEMPERATURE.

A subject of much importance seemed to me to be open for inquiry in respect to the conditions of the animal temperature under the action of those methyl and ethyl compounds which produce temporary insensibility. Attempts had already been made by myself and others to determine the order of facts from the human subject. But the circumstances under which the narcotizing agents are administered to human kind are so peculiar that no separate observer could conduct a satisfactory research in a sufficient number of cases to be reliable. To arrive at correct data, a period of three hours at least is required for each observation; the air of the room in which the observation is made must be kept at a steady and uniform heat; three observers are also wanted, and the utmost quiet must prevail, that excitement of an external character do not move or annoy the subject. To meet these necessities, I determined to conduct the research on inferior animals. I selected from two kinds of animals,—the guineapig, whose temperature is not more than two degrees above man, and the pigeon, an animal which has a mean temperature full ten degrees above that of man. I next selected chloroform and ether as the two

agents to be tested; it was necessary, from the labour and time demanded, not to take more than two substances, and these I considered were fair representatives of two distinct series, while at the same time they were substances with which the world of science generally is most familiar.

I had the pleasure of being most ably assisted in observation by my friends Dr. F. Versmann and Dr. Sedgwick, with Mr. Alfred Nutt occasionally, and Mr. Alfred Haviland; in every case another pair of eyes besides my own kept strict watch. The thermometers used were by Casella. The plan of proceeding was first to bring the air of the laboratory to a fixed temperature, the mean of 54° Fahr. being carefully sustained. In this air the animal was allowed to live for two hours before the observation, and the natural temperature of the animal was carefully noted—in the pigeon from the cloaca, in the guineapig from the rectum.

The animals were put to sleep by being made gently to inhale the vapours to which they were subjected from a mask covered with thin flannel, and the thermometer was watched and registered through every stage of narcotism, and for one hour or more after recovery.

In one pigeon the observation was conducted three separate times, in another pigeon twice, and in five other pigeons once. In one guineapig the observation was made three times, in another twice, and in five once.

The phenomena were amongst the most uniform I have ever recorded in experiment.

TEMPERATURE UNDER CHLOROFORM.

In the pigeons subjected to chloroform there was in every case a fall in the thermometer, during the narcotism, of from 5·6° to 6° Fahr. This was so singularly

correct that the result actually varied with the natural temperatures of the birds. Thus in one of the birds, in which the natural temperature was 107.5° , the thermometer came down regularly to 101° ; while in another bird, the natural temperature of which was 110° , the thermometer regularly fell to 104° .

This decline in temperature did not date from the actual commencement of inhalation. In the first stage the thermometer remained steady; as the second stage advanced it rose, on an average, half a degree, to sink at once as the excitement passed away; while as the third stage was reached (with entire loss of sensation and of motion) it fell from three to four degrees. As the third stage became more determined, and the fourth approached, the decline continued, the minimum of six degrees being marked by all the signs of impending death, viz., feeble breathing, intermittent pulse, and complete muscular relaxation. At this extreme point recovery was permitted. The temperature during the recovery began in all cases to rise in from two minutes and a half to three minutes after the chloroform was withdrawn: it would even rise one degree in this period, and would continue to ascend with the same rapidity for as many as three degrees. But the last three degrees were regained always slowly, as long a time as an hour occurring for the progress of each degree; this was particularly the fact where the temperature at the beginning was very high. The bird whose temperature was 110° was three hours recovering four degrees of heat, and nine in recovering the full quantity lost, 6° . One symptom common to the action of chloroform (I mean vomiting) exerted a marked and sudden change, causing a rapid fall in the mercury of, frequently, two degrees. The change was not observed during the act or strain of vomiting, but instantly afterwards. The

loss of heat thus sustained was not made up so long as the inhalation and the narcotism were continued.

TEMPERATURE UNDER ETHER.

Under ether the decline of temperature in pigeons is not so determinate as under chloroform, but it is continuous, *i.e.*, without any temporary rise, from the first. The fall amounts to an average of four degrees. The absence of vomiting during administrations of ether may account for this difference to some extent; but the grand reason why the temperature keeps up so well is the quietude that prevails, the avoidance of that muscular excitement which so prominently distinguishes the action of the chlorides.

In the administration of chloroform to guineapigs the same changes of temperature were observed, but in a less marked degree. The temperature during the stage of excitement rose 1° Fahr., and then sank three degrees, that is to say, 2° below the natural standard. The temperature also remained at the lower degree during the whole period of recovery, and did not return to the natural condition for a period of nearly four hours.

Under ether the temperature of rabbits showed extremely slight variation. There was no preliminary increase of temperature, and under the deepest anæsthesia the reduction never reached beyond one degree and a half, with return to the natural standard in an hour.

NARCOTISM UNDER A HIGH TEMPERATURE.

In two short series of final experiments on temperature I inquired whether any extreme changes or any differences would result by artificially increasing the temperature of the air in which the animal inhaled the vapour of

chloroform. A pigeon, having a natural temperature of 108° Fahr., was placed in a warm-air bath at 112° Fahr.; in a short time the temperature of the animal rose three degrees, where it remained stationary, the further accumulation of heat being arrested by copious elimination. The animal was now put to sleep with chloroform, the sleep being induced with extremest rapidity, as is common in high temperatures. During the passing interval of excitement the thermometer remained steady, and then suddenly fell to 109° . The vapour was withdrawn at this point with a return to sensibility that was startling from its rapidity. Narcotism once more induced, the thermometer went down to 107° , but rose once more almost instantly to 109° , on the vapour being withdrawn.

In another observation on a pigeon, having a natural temperature of $108\frac{1}{2}^{\circ}$ at 60° , the temperature of the animal, in a room at 96° , rose to 110° Fahr. During narcotism in this state the temperature fell to $108\cdot5^{\circ}$, $107\cdot8^{\circ}$, 107° , and then suddenly to 105° , when insensibility was complete. As recovery progressed, the temperature rose to $108\cdot4^{\circ}$, but during a slight vomiting fell again to $107\cdot7^{\circ}$. After this the temperature rose to 108° , and remained there, the animal seeming entirely well.

NARCOTISM UNDER A LOW TEMPERATURE.

In a further short series of experiments the influence of cold during narcotism from ether-vapour was tested. The temperature of a pigeon having been reduced by narcotic action of ether from 107° to 104° , the spray of ether was directed upon the head until the superficies of the cerebrum were solidified. Under these combined influences the temperature of the body ran down as low as $97\cdot7^{\circ}$. At this point of extreme reduction of heat there occurred a severe tremor; but on restoring warmth,

the recovery was steady and good, the natural heat being back in ten hours.

In a similar experiment made with chloroform-vapour, the results were the same within a little less than a degree. The reduction of temperature was from 107.5° to 98.5° . The recovery was good.

One other experiment deserves notice, from the accident attending it. In administering chloroform to a rabbit having a temperature of 102° , the thermometer, during a sharp paroxysm of muscular excitement, rose at one move to 103.8° , or nearly two degrees. At the moment the heart ceased to pulsate, and the animal was dead. The occurrence is entirely in character with what has often happened in the human subject under chloroform. The heart involved in the sudden muscular excitement has contracted spasmodically, and the motion of life has ceased for good.

I ought almost to apologize for relating these facts on the temperature of animals under chloroform and ether with so much minuteness; but practical physiologists, who will at once see their bearing, will pardon me. The facts we have gathered are:—

- (a) That during the stage of the muscular excitement incident to the circulation of chloroform through the body there is a temporary increment of heat.
- (b) That with the cessation of the muscular motion there is a gradual decrease of heat.
- (c) That the decrease of temperature is greatly accelerated and intensified by the act of vomiting.
- (d) That coincidently with the minimum of reduced animal temperature there is intermittent action of the heart and the extreme of muscular prostration.

INFLUENCE OF ELECTRICAL EXCITATION DURING THE
ACTION OF METHYL AND ETHYL COMPOUNDS.

The observations already recorded led me to make a further inquiry in respect to the influence of the galvanic current at different stages of narcotism. In this research chloroform was the only narcotic used, and pigeons were the subjects of experiment.

In the first experiment, the animal having been narcotized by the vapour of chloroform until the development of the stage of excitement, and the consequent increase of temperature, a needle connected with one pole of an electro-magnetic machine was passed through the skin of the neck, while a second needle was passed through the leg. The free end of the second pole was next brought to the needle in the leg, and a moderate current was passed through the body. In an instant the muscular over-action was converted into general rigidity, the chest was fixed, the heart was fixed, and death was the immediate result.

In the next experiment, the same arrangements for the transmission of the current having been made as before, and the same strength of current being used, the other details were changed. The animal was allowed to pass through the stage of muscular excitement into the third stage of muscular relaxation. This stage reached, the current was transmitted through the body of the animal. The effect, again instantaneous, was not to destroy life, but to excite all the phenomena of apparent active life. The wings were moved as in the act of flight, motions attended with progressive or propulsive action were strongly marked, the breathing was quickened, the impulse of the heart was increased, and the temperature rose; but when the current was withdrawn all these striking

phenomena were withdrawn also, and the animal again slept inactive and prostrate, but ready to recover.

For the sake of comparison another experiment of a similar character was carried out, in which, instead of reducing the animal body by the administration of chloroform, the spray of ether was directed upon the cerebrum until the phenomena of entire unconsciousness and of muscular prostration were fully pronounced. Then the current from the coil was passed through the body as before, with the temporary development of the phenomena of muscular activity.

The lessons taught by these experiments are in confirmation of all that has gone before. They prove how distinct a double action on the muscular organs is exerted by chloroform, and they indicate an independency of influence upon the muscular and nervous systems, between insensibility, or I had better say unconsciousness, and deficiency of motor power, which I was not myself, previous to the experiments, prepared to recognize. It is clear that in the narcotism induced by chloroform, and perhaps by all other similar agents, the muscles can be called into action while the brain is virtually dead; we can, in short, supply the muscles with an artificial nervous energy.

DANGERS ATTENDING ADMINISTRATION OF METHYL OR ETHYL COMPOUNDS.

Recasting these experiments and putting the various facts in order, I have been led to consider from them the dangers which waylay the administrators of the methyl and ethyl compounds, the causes of these dangers, and the way to avoid them.

And here, I think, is a primary truth, the basis of progress in the discovery of new agents:—that the

danger in anæsthesia does not lie in the production of sleep, nor even of deep sleep, but in the production, in the course of the process, of symptoms which, although the prime sources of danger, are not connected by any necessity with the anæsthesia. I refer especially to the symptoms of vomiting and muscular excitement and rigidity, followed, as we have seen, by decrease of temperature of the animal.

Again, I think there is another truth hardly secondary, viz., that we already possess agents which by their action prove to us their power of producing anæsthesia without exciting muscular rigidity, and without materially disturbing the animal temperature. If this be true, we ought as men of science to exclude at once from the list of safe anæsthetics all such as are found, in experiments, to produce rigidity of muscle or vomiting (which is an indication of the same action), and reduction of heat by its transference into motion at a moment when the conditions for the liberation of force are most unfavourable.

The exclusion here named would, I conceive, of itself save many lives, and would bring the danger of artificially induced sleep to the danger, and no more, of mere natural sleep. The only cause of accident would be in carrying the insensibility to the extreme of extinguishing life, an accident for which the administrator would be clearly culpable, and which, even now, when very dangerous agents are in daily use, has never, as far as I know, happened.

In searching for the agents we require, we must begin by excluding bad ones, which is, in fact, to exclude whole classes. The class of the chlorides, under this rule, would all go. It is true they are not all equally dangerous, and that the increase of danger is in proportion to the substitution of chlorine; but, as a class, they one

and all do more than we require, they produce muscular rigidity, vomiting, and decrease of animal heat. This same rule holds good in relation to the iodides and the bromides. Supposing, then, we keep to the methyl and ethyl series as bases, we are driven back to the oxides, to what are commonly called the ethers, for our agents. None of these which have yet been applied have been actually perfect; but it is almost certain that in course of time the chemist will produce for the physiologist the precise requirement. That it may be generally known what this requirement is, I will state in a few sentences the theoretical formula of a safe anæsthetic, an anæsthetic that shall be applicable to long and short operations alike, and shall become acceptable generally from its readiness and safety.

1. It must be a fluid. Gases, however good, are not practicable as agents for general and daily use; more than this, at the temperature of the blood they remain as gases, and when dissolved in blood they exert no action from change of form in the organism. To narcotize with them it is consequently necessary to give them in large quantities, even it may be to the exclusion of air altogether. The influence of a gas thus administered is of necessity limited to the briefest interval of time; steady continuance would lead to certain death from asphyxia.

2. The fluid must possess homogeny and stability. Mixtures of fluids are utterly unreliable. Fluids which easily decompose under the influence of heat or of light are unreliable.

3. The fluid must be of pleasant odour, and must produce no irritation when inhaled or when applied to the skin. All the fluids which, like chloroform, amylene, and turpentine, cause redness and irritation of skin, cause

also, when introduced into the blood, irritability of muscle and rigidity, together with vomiting.

4. The boiling-point of the fluid should be not less than 110° Fahr., and not over 130° . Fluids which pass into vapour below the temperature of the animal body act practically as gases, and must be used in free quantities to the exclusion of air; common ether, amylene, and bichloride of methylene have this fault, the last least. Fluids, on the other hand, which pass altogether into vapour at a point much above the animal temperature, as at 140° or upwards, condense in the pulmonary blood too determinately, and although they create a long sleep, they remain for a considerable period in the body, creating continued nausea and depression from interference with the conservation of the animal heat. Chloroform, tetrachloride of carbon, and common alcohol are objectionable on these grounds. A fluid which should have a boiling-point some 20° above the body, and other properties equally good to commend it, would be convenient in every respect. It could be easily preserved, and as an anæsthetic it would be alike applicable to long and short operations.

5. The density of the vapour of the fluid should be about 40, taking hydrogen as unity.

COMMENTARY AND SUPPLEMENT, 1890.

The Report rendered above was written and originally published twenty-two years ago. Reconsidering it from what has transpired since, I feel every reason to be satisfied with it, and with the practical lessons it conveyed. It explained the effect of external temperature on the administration of chloroform, showing that increase of temperature quickens action, and it illustrated very forcibly the fact that on the lower animals the

influence of chloroform, after the inhalation of its vapour, arrests the function of respiration before it interferes, dangerously, with the work of the heart. These same facts seem to have been elicited by the Hyderabad Commission; and it is important to know that they have been so far confirmed, although it could not well be otherwise, seeing that every detail was derived from the strictest observation of natural phenomena. One further fact may, however, be deduced from these original observations, a fact which does not appear in the late discussions, namely, that the presence of chloroform in the muscular system excites the muscles to contraction, and even to tetanic spasm, by the direct irritation which it promotes. From that fact we may draw an inference, amounting almost to a certainty, that in cases of irritable heart, attended with feebleness, a common combination, there may be introduced, through the lungs, a sufficient percentage of the chloroform vapour to excite the heart into contraction by a direct effect. In such instances chloroform may kill by an immediate influence over the heart, and by a mode of death quite distinct from that reflex off the bronchial surface, to which I have drawn attention in later years.

In supplement of the observation on absorption of anæsthetic vapours by the lungs, it is proper to add in this place, what I demonstrated in 1868, that there is such a thing as a direct absorption of chloroform vapour, and probably of other vapours or gases, through the olfactory nervous expanse. In the lower animals I have seen deep narcotism sustained from an inhaler charged with chloroform and held to the nostrils whilst respiration with common air was being carried on through a tube fixed in the trachea. This is an important practical truth, because it suggests that whilst the blood is becoming

charged with chloroform during ordinary inhalation, the cerebral matter may also be undergoing charge through the olfactory tract, and that reflexes may be taking place directly from the brain itself as a poisoned centre.

A third and supplementary recommendation may be offered, bearing on the solubility of anæsthetic vapours and gases, when they are under study, with the idea of obtaining from them a model anæsthetic, after the manner described in ASCLEPIAD, vol. ii., pp. 361-78, for 1885. If a gas or vapour be too soluble in the blood it is liable to produce too rapid an asphyxia; if it be too insoluble it is liable to act after the manner of a gas injected into a vein, and to interfere, mechanically, with the course of the blood through the minute circulation. In one experimental observation on a lower animal, in which insensibility was quickly produced by the administration of the vapour of the insoluble amyl hydride, death took place suddenly, and on examination, immediately afterwards, the veins were found filled with blood charged with the vapour of the hydride so freely that the vapour could be ignited from a lighted taper. This question of the proper solubility of a general anæsthetic has never yet been correctly investigated, and I am myself only able to state that while ether is too soluble, chloroform is too insoluble. The mean of the two would be nearer the mark.

PULMONARY CONSUMPTION.

IN many parts of provincial England it was once a practice, hardly yet abolished everywhere by the common people, to treat persons affected with consumption of the lungs by making them swallow live snails. The vulgar idea of the *modus operandi* of the measure was, that the snails found their way to the lungs of the patient and ate the black worms which lived on the lungs and caused, by their depredations, the disease. The snail treatment got a great reputation, for which probably it depended on two collateral causes. The persons who were benefited by it were led to seek the remedy in purer air than they had been accustomed to live in; and in addition to the snail, which did neither good nor ill, they partook freely of new milk and cream, rich foods likely to be important adjuncts to the cure. Of late the idea that pulmonary consumption is a parasitic disease is revived. The lung infested with hosts of a specific bacillus is supposed to be destroyed by them or by the secretion which they eliminate; and the mode of cure is expected to be found in a lymph or poison which shall kill the bacillus without injury to the patient.

At the present moment the general press is filled with reports and anticipations of such a kind that the uninitiated in the mystery of disease are beginning to see an immediate future when phthisis pulmonalis, which next to alcoholic disease stands highest as a cause of

mortality, will practically be wiped off the blackboard of death, and one of the most fatal maladies of past ages be known no more. This were indeed a consummation devoutly to be wished, and he would be a most unjust steward to medical trusts who would not magnify a mode of cure proving to be one of the grandest gifts the art of medicine has ever bestowed on mankind. But scientific medicine is conservative and careful before she speaks a word; and the more so because in her too ready acceptance of speculative practices in former days she has seen reason to become wary and chary of belief. Some of her children run after new remedies and false gods fast enough; and many, no doubt, will run after this new promise long before there is sufficient evidence either for its establishment or its delegation to the shades of departed fallacies. Letting these pursue their own course for success or discomfiture, the prudent will wait not weeks nor months only, but years, before they come to a conclusion that shall mean an affirmative and historical success or failure. For the success of the mode of treatment, attributed to Dr. Koch, but claimed already by some rivals of his school, can be proved by one proof alone, namely, the effect of the practice on the death-rate.

Up to this time pulmonary consumption has had a high though declining death-rate, on which no defined line of *curative treatment* has had any obvious effect. The figures which indicate the death-rate will show, therefore, from date, the effect of the new mode of treatment.

In order to arrive at perfectly correct returns, many details will, however, have to be taken into account. First and foremost, it must be remembered that Consumption is now on the decline from sanitary improvements alone.

In 1881 the late Sir Edwin Chadwick joined me in making a calculation, by which we estimated the mortality from consumption in England in the year 1880, in order to compare it with the mortality of the disease in England during the year 1847, the space of thirty-three years being marked by moderately advancing sanitation. From that comparison we learned that pulmonary consumption had decreased in the proportion of three and a-half per cent. of the deaths from all causes; that is to say, 12·57 persons died from it in 1847 to 9·12 in 1880. During the past ten years the decrease has been going on at a more rapid rate, although the figures are not yet at command. This must all be taken into account here, in estimating the assumed evidence of benefit from new treatment of a specific order.

To some minds it will occur that the proofs ought to come directly from the clinical results. The expectation is fair, yet not altogether without alloy. At first it may be that there will appear a good result even though, in the end, there should be grievous disappointment. We are dealing with hypothesis so far, and hypothesis, leading to practice, invariably carries with it a certain measure of success. Hope is such a flatterer in trials in which hopeful reason is engaged, that, for a time, it misleads the gravest, touching sometimes the pessimistic fraternity itself. The true tests of success will have, in fact, to meet many diversions and obstacles. Those of us who remember the introduction of cod liver oil into the treatment of consumption will realize all these difficulties. That famous treatment came to us on the highest authority. Moreover, it stood on a plausible theory, and at first cures upon cures were attributed to it. In course of time it turned out that, although the treatment had its value, it was no specific. The cases it was supposed to cure were good

simulated cases of bronchial phthisis, not the veritable affection; cases which might have recovered, under favouring circumstances, without medicines, and which advertise the innumerable empirical "cures."

In the tests that will have to be applied to the new era, as some call it, every kind of doubt will have to be overcome. We must know in what we believe. We must know whether the proposed new method is applicable to all types of the affection: to alcoholic phthisis, which strikes down the middle-aged, as well as to the encroaching malady which steals on the predisposed young. We must discover whether the predisposed are as favourably influenced as those free of constitutional taint.

Is the youth with delicate lung, the pigeon-shaped chest, and the hectic cheek, in whom the physical disease is pronounced, as amenable to the cure as the more favoured person who comes under its influence? Granted, for argument's sake, that one attack is cured by the new treatment: how long will the cure hold good? Will the predisposed person, safely cured, be warranted from a renewed attack if he or she go out into the world, "take cold," and suffer from that localized pneumonia or acute tuberculosis which older observers look upon as the origin, in nine cases out of ten, of the fatal malady?

THE LATEST REPORTS ON THE NEW PROCESS.

Whilst the above notice of the newly-proposed cure for consumption has been in the press, an official preliminary report has come from Berlin, supplying a certain number of details which call for serious notice. This report tells us that the new remedy is administered by subcutaneous injection, and that it is valueless when administered by the stomach. Whether it is a pure lymph or a compound of lymph with some chemical substance, or a chemical

substance altogether, we are not informed, the inventor of it, Dr. Koch, wishing to keep the composition a secret for some time, in order that he may perfect it, if under experience it calls for modification. The substance is a brownish liquid, 0.25 cubic centimetres of which suffice to produce an intense effect. It is more active on man than on the guineapig, the 1,500th part of the quantity which has no appreciable effect on the guineapig acting powerfully on the human being. The symptoms produced by the substance commence about three hours after the injection of it, and consist of pain in the limbs, fatigue, and inclination to cough, followed by ague, sickness, vomiting, and rise of temperature to 39.6° C., or 103.2° Fahr. The symptoms abate in about twelve hours.

The specific action of this treatment is claimed to be on tuberculous tissue only. It does not act by killing germs; it can only influence living tuberculous tissue, which tissue it kills, not other healthy or even diseased tissue. It claims to cure pulmonary consumption by destroying the tuberculous material, the *materies morbi* of tubercle,—a retreat back to the old theory of treatment, and dead against the so-called germicide method. But it is also claimed for it that it cures lupus, and therefore we must assume that lupus is tuberculous; in a word, whatever it cures is of tuberculous type, a tremendous leap.

The people appear to be accepting all that is said as absolute proof. To the popular mind, at this moment, tubercular disease is near its end in the world. A physician who has had forty years of experience on this subject must not be misunderstood if he cries "Wait and see!" before accepting so unqualified a decision. Some remarkable results may take record at first; but what next? We who are accustomed to observe without burning enthusiasm, are forced to remember results which

have astonished calculations derived from experiences in the main sound and certain. We see sometimes in practice certain spontaneous cures or recoveries. I have recently seen a young lady patient, whose sister died of phthisis, and who, two years since, was stricken with every indication of the disease,—dry crepitation in the left lung, hæmoptysis, wasting, and night sweats, but who has made so perfect a recovery that if I had not, with my own eyes and ears, recognized the symptoms I should almost have doubted the diagnosis. For some time in my practice at the Royal Infirmary for Diseases of the Chest I introduced peroxide of hydrogen as a remedy, and with several such startling results I thought at first a specific had been discovered; but time corrected the hope. Three years ago I treated an extreme case of lupus of the face with sodium ethylate: scales formed on the red ulcerated surface with free exudation of serous fluid, followed by complete recovery. It seemed specific treatment; but in three succeeding cases the same mode of treatment rendered no such effective service, although in one for a time there was the greatest promise. Under this new treatment shall we see similar successes and failures? Once more. We shall require to know in what manner the remedy operates. Is it through the nervous system, or is it by some chemical or chemico-vital action which it exerts in contact, in infinitesimal division, with the tuberculous living substance?

By the time the next ASCLEPIAD is due some measure of answer to these inquiries will probably be to hand, and by then we shall possibly be informed, as we ought to have been from the first, of the composition of a remedy possessing the virtues claimed for it under the bare name of *paratoloid*.

COTEMPORARY PRACTICE AND LITERATURE.

“Every physician will and ought to make observations from his own experience ; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together.”

DEATH UNDER METHYLENE, AND MODE OF ADMINISTRATION.

HE recent death of a man under narcotism from Methylene at the Plymouth Hospital has caused much attention to be paid to the subject of mode of administration. It appears from the report of the House Surgeon that the narcotic was administered from Rendle's inhaler, a funnel-shaped tube, into which the methylene was freely poured, and from which a large percentage of the vapour must have been inhaled. This apparatus has been in operation for a long time, and has often led to the necessity of using an ounce of methylene, at least, during one administration. I was once for a short period inclined to view with favour Rendle's inhaler, and I employed it several times without any serious result ; but I became at last convinced that it was not uniform in its action, and that it caused a very unnecessary loss of narcotic fluid. These facts led me to lay by not only Rendle's apparatus, but one also which I had myself constructed, in order to give Dr. Junker's inhaler a fair trial ; and now for twenty years past I have used none other, being satisfied that, taking it all in all, it is the safest, the most economical, and the best. Since

employing this inhaler I have never had occasion to use a fluid ounce of methylene for the longest operation. I once actually sustained good anæsthesia for an hour and twenty minutes with six fluid drachms; and, over and over again, I have found two fluid drachms sufficient for an operation lasting half an hour. Recently Messrs. Krohne and Seseman, who originally constructed the inhaler for Dr. Junker, have added improvements, which make the instrument more perfect. They have introduced a regulating stopcock, and a delicate feather indicator, which shows, during administration, the respirations of the patient during every stage. These are most useful additions.

Returning to the fatal administration at Plymouth, I fully agree with the opinion expressed by Sir Spencer Wells in the *Lancet* of October 24th, that the mode of administration was much less safe than by the Junker apparatus. At the same time, it is only just to say that, as the *post mortem* showed, the unfortunate patient might easily have died under any mode of administration, and under any anæsthetic. He had declared heart disease,—degeneration,—liver disease, spleen disease, kidney disease, and if minute investigation of the nervous system had been instituted, disease, most probably, in both the ganglionic and the cerebro-spinal centres. Added to this, he had long passed the average duration of life for one of his class, a day labourer. In a word, he belonged to the *morituri*, as one ready to die under any operation, oppression, or shock to which he might be subjected. This fact does not say a word against precaution in the mode of administering methylene and other anæsthetics; on the contrary, it emphasizes precaution, but it should be remembered as the leading cause of this fatal event.

"GOLDEN BULLETS."

[BY WILLIAM W. IRELAND, M.D.]

NDER the above title the author of the two remarkable books, "The Blot on the Brain" and "Through the Ivory Gate," has written a story belonging to the days of Akber and Elizabeth. This is not the first attempt Dr. Ireland has made in the line of fiction, and it is to be hoped it will not be his last; but it is his best. He challenges, in his preface, students of the history of life in India to say whether his picture of it, in the most brilliant period of Mussulman rule, is a true one or not; and he adds that as the book is not designed, solely, for persons who have special relations with India, and, as interest goes along with knowledge and is fed by it, so he hopes that "many who are willing to learn something about our Eastern Empire, in a pleasant way, will turn to these pages; and, if they gain from them a more vivid conception of a memorable time, they will not refuse to believe that they have learned something."

The story opens in the year 1598, and relates the adventures of a young Englishman named Stephen Ashbourne, the son of a goldsmith in the city, who set out from Gravesend in a good ship called *The Unicorn*, a ship of four hundred tons, commanded by Captain Edward Hawkhurst, and manned by eighty-two able-bodied seamen, with fourteen guns and a rich cargo, under charge of Ashbourne, to find the way to some Indian port where a profitable commerce might be opened. Through the Earl of Essex the captain procured from Queen Elizabeth, and carried with him, letters from Her Majesty to divers princes in India, as Sir Hugh Willoughby and Captain Chancellor had, in a previous reign, carried letters from

Edward VI. to divers princes whom they might approach in their adventurous voyages. Thus prepared, the adventurers of *The Unicorn* finally reached the port of Surat, not without a fight with a Portuguese carrack, which, owing to Ashbourne's skill in gunnery—for he added to his other qualities a mastery of artillery—came out of the encounter badly maimed and beaten, though not captured.

It would deprive the reader of "Golden Bullets" of all his pleasure were the details of the adventures of Stephen Ashbourne mentioned in this place; were the results and ends of his life named, or the reason for the title of the book suggested. The novel reader finds his own independent pleasure in his study of the novelist, and he is, indeed, a selfish reviewer who pads his own page with the wit of the author he reviews. All, therefore, that shall be revealed here, after a reading of the book twice told, with twice told pleasure, is that the pictures of life in India, at the moment when English eyes were first set upon it, and the crown of the Empress of India was in embryo, are rich in information; that the story of adventure and of enterprise holds on all through the narrative; that a pure and chastened interest of love is recorded with a charming romance surrounding the lovers; and that all is so pointedly told it seems as if it could not be a novel, but must be a real history, written by one who was present in the making of it. As a contrast to the childish frivolity of the popular novel now floating in the book market, "Golden Bullets" is great indeed. There must surely be a reading class to whom this contrast will be a joy as well as a relief, and from whom a knowledge of the work will be disseminated with the honourable and useful motive of giving to good art in literature its true value and quick currency.

THREE EVERY-DAY BOOKS.

Neale's Digest.—Attention has more than once been called in these papers to Dr. Neale's admirable "Digest," and all practitioners who wish to save their time will be glad to hear that the work, if properly supported, will soon appear in a new and enlarged edition, including half a century's survey of medical literature in all its branches, and supplying to its readers a clear reference to every subject they may wish to make a note of at a moment's warning. In some instances the reference will lead to the original paper wanted, in others it will itself convey the information that is required. In this respect the book will be of immediate value to the busy man of the time; but the value will not end there. It will extend to the medical scholar of the future, who will find in Dr. Neale's pages an *index rerum* feeding him with the history of the days of his medical forefathers. The new "Digest" will find, it is to be hoped, a prominent place in every medical library.

Martindale and Westcott's Extra Pharmacopœia.—This useful handbook has reached its sixth edition. Fifty pages have been added to it, and the therapeutical references and index have been largely extended. If the authors of this popular little volume, of which thirty-six thousand have been issued, go on as they are going, they will bid fair to make their Pharmacopœia the Pharmacopœia *par excellence*: convenient, clear, and all-sufficient.

Smith's Visiting List for 1891 in its forty-fifth year is as young as ever in tone, and retains its universal utility, as a pocket companion, unimpaired.

"THE CRIMINAL."

[BY HAVELOCK ELLIS.]

THIS is an illustrated work (Walter Scott & Co.) of an exceedingly instructive and interesting character, intended to present to the English reader a critical summary of the results of the science called Criminal Anthropology. The author claims to have devoted fifteen years to these studies ; and while admitting that nothing original has been advanced in this volume, expresses that it represents a very large body of intelligent opinion in many countries. He opens with a chapter enumerating the varieties of criminals ; then he passes to the study of the criminal, proceeding onwards to criminal anthropology, physical and psychical. A chapter on results of this study follows, with another on the treatment of the criminal, and finally a series of conclusions. The illustrations are remarkable, and the book altogether commends itself as much to the medical as the general scholar.

TO CORRESPONDENTS.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD is exclusively by the Author whose name is on the title-page.

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD should be addressed to the Author at his residence, 25, Manchester Square, London, W.

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, Paternoster Square, London, E.C.

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

November
15th,



1890.

INDEX.

A.

PAGE

Acute Poisoning by Chloral Hydrate	240
Additional Studies on Chloral Hydrate	245
"Alcoholism, Chronic" (JOSEPH F. PAYNE, F.R.C.P.) Review	192
Amyl, Nitrate of	283
Amyl, Nitrite of	68, 178, 281
Artificial Respiration, The Theory and Practice of	201
Asphyxia, Apnoea, and Syncope of the Lesser Circulation	101
"Asphyxia" (GEORGE JOHNSON, M.D., F.R.S.). Review	194

B.

Bell, Benjamin, F.R.S. Edin. (<i>with Portrait</i>)	336
Black, Joseph, M.D. (<i>with Portrait</i>)	254
Blood-letting for Reduction of Fever Heat	144
Borborygmus, Voluntary	145

C.

Carbolic Colloid.	335
Carbolic Ether Spray	252
Chloral Hydrate, Acute Poisoning by	240
" " Symptoms of Poisoning by, ...	243
" " Additional Studies of	245
Chloroform, Death by	1
Cotemporary Practice and Literature	88, 190, 294, 384
Criminal," "The (HAVELOCK ELLIS). Review	392
Cullen, William, M.D., F.R.S. (<i>with Portrait</i>).	148
Cycling and Physique	21

D.

	PAGE
Death by Chloroform	1
Death, Diaphanous Test of	251
Death under Methylene, and Mode of Administration	387

E.

Ear, External, Removal of Impactions from	47
Electricity, Execution by	291
Epidemic, Hypnotic, the	118
Epidemic of Influenza, and Pneumo-Paresis	40
Epistaxis, Notes on	139
Ethyl and Methyl Compounds	361
"Extra Pharmacopœia" (Martindale and Westcott). Review	391

F.

Fasting Mania, Lessons from the	225
---	-----

G.

Galvanism and Resuscitation	44
"Golden Bullets" (WILLIAM W. IRELAND, M.D.). Review	389

H.

"Hygiene and Public Health" (LOUIS E. PARKER, M.D.). Review	294
Hypnotic Epidemic, The	118

I.

Impactions, Removal of, from the External Ear.	47
Influenza, Sequelæ of	136
'Irish Medical Practitioners' Guide" (PROF. R. J. KINKEAD). Review	93

L.

Lessons from the Fasting Mania.	225
Local Anæsthetic Sprays, Carbolic Ether Spray	252

M.

"Manual of Anatomy for Senior Students" (EDMUND OWEN, M.B., F.R.C.S.)	
Review	196
"Manual of Pharmaceutical Testing" (BARNARD E. PROCTOR). Review	297

	PAGE
Medical Practitioners' Guide," "The Irish (PROF. R. J. KINKEAD). Review.	93
Meteorological Station, Sick Ward as a	248
Methyl and Ethyl Compounds, Report on	361
Monro, Alexander, M.D., F.R.S. (<i>with Portrait</i>)	49

N.

Neale's Digest (R. NEALE, M.D.) Review	391
Nitrate of Amyl, Physiological Properties of	283
Nitrite of Amyl, Physiological Properties of	68, 178, 283

O.

Opuscula Practica	40, 136, 240, 328
Oesophagus, Water Gurgle in Diagnosis of Position of Stricture of the	332

P.

"Physiognomy; or, How to Read Faces" (MARY OLMSTED STANTON). Review	190
"Provision for the Insane Poor" (D. HACK TUKE, M.D.). Review	92
Pulmonary Consumption	381

R.

Reports, Physiological and Therapeutical, made to the British Association for the Advancement of Science from 1863 to 1872:—	
On Physiological Properties of Nitrite of Amyl	68, 178, 281
" " Nitrate of Amyl	283
" " Compounds of Methyl and Ethyl	361

S.

Sense of Safety	333
Sick Ward as a Meteorological Station	248
Smith's Visiting List. Review	391
Symptoms of Poisoning by Chloral Hydrate	243
Superficial Veins, Large, of the Extremities	330

T.

Test of Death, Diaphanous, The	251
" " Venous Turgescence, The	46
Testing, Manual of Pharmaceutical," "The	297
Three Every-day Books. Review	391
"Through the Ivory Gate" (WILLIAM W. IRELAND, M.D.). Review	295
Toe-Nail, Treatment of Ingrowing, The	334

V.

	PAGE
Vaccination," "History and Pathology of (EDGAR M. CROOKSHANK, M.B.)	
Review	88
Veins, Large Superficial, of the Extremities	330
Venous Turgescence Test of Death	46
Voluntary Borborygmus	145

W.

Water Gurgle in the Diagnosis of the Position of Stricture of the Œsophagus	332
---	-----

APPENDIX OF ACKNOWLEDGMENTS.

- AUTHOR** of "Celtic MSS. in Relation to the Macpherson Fraud." By **AUTHOR** of "CRITICISM A MYTH." London : E. W. Allen. 1890.
- BAUDUY, J. K.** "Insomnia." By J. K. BAUDUY, M.D., LL.D. St. Louis. 1890.
- BROWN, A. S.** "Madeira and The Canary Islands." By A. SAMLER BROWN. London : Sampson Low, Marston & Co. 1890.
- BLACKWELL, E.** "Serious Protest sent to the Alumnae Association of the Women's Medical College of the New York Infirmary." By ELIZABETH BLACKWELL, M.D.
- BLAKE, EDWARD.** "Lip Chorea and Puresis of the External Pterygoid Muscles." By EDWARD BLAKE, M.D., M.R.C.S. London : Harrison & Sons. 1890.
- BULKLEY, L. D.** "On the Treatment of Eczema in Elderly People." Philadelphia : W. J. Dornan. 1890. "Dangers arising from Syphilis in the Practice of Dentistry." By L. DUNCAN BULKLEY, A.M., M.D. New York. 1890.
- BUCKE, R. M.** "Sanity." By R. M. BUCKE, M.D. New York. 1890.
- BLACKMAR, F. W.** "History of Federal and State Aid to Higher Education in the United States." By FRANK W. BLACKMAR, PH.D. Washington. 1890.
- BENNETT, CYRIL.** "The Modern Malady, or Sufferers from Nerves." By CYRIL BENNETT. London : EDWARD ARNOLD. 1890.
- BERDOE, EDWARD.** "The Healing Art and the Claims of Vivisection." By EDWARD BERDOE, M.R.C.S. London : Swan Sonnenschein & Co. 1890.
- BUZZARD, THOMAS.** "Address on Vertigo of Bulbar Origin." By THOMAS BUZZARD, M.D., F.R.C.P. London : John Bale. 1890.
- COBBE, F. P.** "The Modern Rack." By FRANCES POWER COBBE. London : Swan Sonnenschein & Co. 1889.
- CATLIN, G. L.** "The St. Gothard Railway." By GEORGE L. CATLIN. Lucerne. 1890.
- CARPENTER, ALFRED.** "Sanitation." By ALFRED CARPENTER, M.D., M.R.C.P. London. 1890.
- CARRINGTON, J. Y.** "Teufel the Terrier; Life and Adventures of an Artist's Dog." By J. YATES CARRINGTON. London : *Pall Mall Gazette* Office. 1890.
- CAINE, W. S.** "No Compensation to Publicans." Two Letters addressed to the Public Press of the Country. By W. S. CAINE, M.P. London. 1890.
- DOLAN, T. M.** "Pasteur and Rabies." By THOMAS M. DOLAN, M.D., F.R.C.S. London : G. Bell & Sons. 1890.
- DUKES, CLEMENT.** "An Address in School Hygiene on Medical Guidance in the Selection of Schools for certain Children." By CLEMENT DUKES, M.D., M.R.C.P. London : Cassell & Co. 1890.
- EARDLEY-WILMOT, R.** "On the Natural Mineral Waters and Spa of Leamington." By R. EARDLEY-WILMOT, M.B. Leamington : W. H. Smith. 1890.
- EVANS, J. H.** "So Very Unexpected." Temperance Readings in Prose and Verse. By J. H. EVANS. London : National Temperance Depôt. 1890.
- FINCH, A. ELLEY.** "The Order of Nature in Relation to Human Life and Happiness." By A. ELLEY FINCH. London : Sunday Lecture Society. 1890.
- FLICK, L. F.** "Special Hospitals for the Treatment of Tuberculosis." By LAWRENCE F. FLICK, M.D. Philadelphia. 1890.
- FRENCH, R. V.** "Nineteen Centuries of Drink in England." By R. Valpy French, D.C.L., LL.D., F.S.A. London : National Temperance League Depôt. 1890.
- FERNALD, JAMES C.** "The Economics of Prohibition." By JAMES C. FERNALD. New York : Funk and Wagnalls. 1890.

- GRUBER, J.** "A Text Book of the Diseases of the Ear." By DR. JOSEF GRUBER. Translated by E. LAW, M.D., M.R.C.S., and COLEMAN JEWELL, M.B., M.R.C.S. London: H. K. Lewis. 1890.
- GORDON, C. A.** "Experiment *versus* Experience in Relation to the Healing Art." By SURG.-GEN. C. A. GORDON, M.D., C.B. London. 1890.
- GILL, JOHN.** "Habits and Health." By JOHN GILL, M.D. London: National Temperance Depôt. 1890.
- HARRIS, ALFRED.** "The Revival of Industries in Ireland." By ALFRED HARRIS. London: King & Son. 1888.
- HARRIS, G.** "Fugitive Efforts in Versification." By DR. G. HARRIS, F.S.A. London. 1890.
- HIRD, J. D.** "The Temperance Reader." By REV. J. DENNIS HIRD, M.A. London: Cassell & Co. 1890.
- HORNIMAN, E.** "Lægevidenskaben og Alkohol-Spergsmaalet." By DR. E. HORNIMAN. Copenhagen. 1890.
- HORSFALL, T. C.** "First Principles of Education." "The Use of Pictures in Schools and Physical Training." Papers read at Manchester Branch of Teachers' Guild. By T. C. HORSFALL. Manchester: T. E. Cornish. 1890.
- JENNINGS, OSCAR.** "Cycling and Health." By OSCAR JENNINGS, M.D. Translated by J. CROSSE JOHNSTON, BRIGADE SURGEON A.N.S. Paris and London: Iliffe & Son. 1890.
- JONES, H. M.** "A few Remarks on Dental Reflexes. On the Etiology of Tinnitus Aurium." By J. MACNAUGHTON JONES, M.D. London. 1890.
- IRELAND, W. W.** "Golden Bullets: A Story in the Days of Akber and Elizabeth." By W. W. IRELAND, M.D. Edinburgh: Bell & Bradfate. 1890.
- JONES, JOSEPH.** "Contribution to Teratology." By JOSEPH JONES, M.D. Address delivered to the Louisiana Medical Society. 1888.
- KERR, N.** "Should Hypnotism have a Recognised Place in Ordinary Therapeutics?" By NORMAN KERR, M.D., F.L.S. London: H. K. Lewis. 1890.
- LONGMORE, T.** "The Sanitary Contrasts of the British and French Armies during the Crimean War." By SURG.-GEN. SIR T. LONGMORE. London: C. Griffin & Co. 1888.
- LITRAGO.** "Grand Canary, the Queen of Health Resorts." By LITRAGO. London: H. Good & Son. 1890.
- MAGUIRE, J. F.** "Father Mathew: A Biography." By J. FRANCIS MAGUIRE, M.P. Dublin: Eason & Son. 1890.
- MASON, L. D.** "Address at a Meeting of the American Association for the Study and Cure of Inebriety." By DR. LEWIS D. MASON. Hartford, U.S.A. 1890.
- MADDEN, T. M.** "The Physician's Place in Gynæcology." By THOMAS MORE MADDEN, M.D., F.R.C.S. 1890.
- MOORE, C. H.** "Development and Character of Gothic Architecture. By CHARLES HERBERT MOORE. London: Macmillan. 1890.
- PHYSIOLOGICAL** Action and Therapeutic Effects of Aperient Mineral Waters." Edited by the "FRANZ JOSEPH" Co. London. 1890.
- PATTESON, R. G.** "Trichomycosis Nodosa; A Bacillary Disease of the Hair." By R. G. PATTESON. "Contribution to the Study of Intra-cranial Complications of Chronic Otitis Media." By R. G. PATTESON, M.B., F.R.C.S.I. Dublin: John Falconer. 1889.
- REMONDINO, P. C.** "The Marine Climate of the Southern Californian Coast in Relation to Phthisis." By P. C. REMONDINO, M.D. 1890.
- SANDERSON, J. B.** "Photographic Determination of the Changes which take place in Muscle during the Period of so-called Latent Stimulation." By J. BURDON SANDERSON, F.R.S. 1890.
- SCHOFIELD, ALFRED.** "Health at Home Tracts." By ALFRED SCHOFIELD, M.D., M.R.C.S. London: Religious Tract Society. 1890.
- STEWART, C. J.** "Notes on the Climate and Weather during the Summer Months at Great Yarmouth." By REV. C. J. STEWART, M.A. Great Yarmouth. 1890.

- STEAVENSON, W. E.** "The Use of Electrolysis in Surgery." By W. E. STEAVENSON, M.D., M.R.C.P. London : J. & A. Churchill. 1890.
- STONE, G. J.** "On the Relation between Natural Science and Ontology." By G. JOHNSTONE STONE, M.A., F.R.S. Dublin : Ponsonby & Weldrick. 1890.
- STURGES, OCTAVIUS.** "Natural History and Relations of Pneumonia." By OCTAVIUS STURGES, M.D., F.R.C.P., and SYDNEY COUPLAND, M.D., F.R.C.P. London : Smith, Elder & Co. 1890.
- WEBB, JAMES.** "Phrenological Aspect of Modern Physiological Research." By JAMES WEBB. London : L. N. Fowler. 1890.
- WELLS, R. J.** "English-Eskimo and Eskimo-English Vocabularies." Compiled by ENSIGN ROGER WELLS, jun., and INTERPRETER JOHN KELLY. Washington. 1890.
- WELLS, T. SPENCER.** Address on Department of Medicine. By SIR T. SPENCER WELLS, Bart. Manchester : Cornish. 1890.
- WHITE, F. E.** "The Evolution of Morals." By FRANCES EMILY WHITE. Chicago. 1889.

PROCEEDINGS AND TRANSACTIONS.

- "Proceedings of the Society for the Study of Inebriety." August 1890.
- "Proceedings of the Medical Society of London." Vol. XII. London : Harrison. 1889.
- "Proceedings of the Department of Superintendence of the National Educational Association at its Meeting at Washington, March 1889." Washington. 1889.
- "Transactions of the Sanitary Institute." Vol. X. Congress at Worcester. London. 1890.
- "Tenth Annual Report of the State Board of Health of New York." Albany : J. E. Lyon. 1890.
- "Report of Benevolent Society of St. Patrick." London : J. Truscott. 1890.
- "Report of the Detroit Emergency Hospital." Edited by HAL. C. WYMAN, M.S., M.D. Detroit. 1890.
- "Proceedings of the Physiological Society." 1890. No. IV.
- "Report of the Council of the After-care Association." 1889 and 1890.
- "Annual Report of the County Lunatic Asylum at Berry Wood, Northampton, ending Dec. 31st, 1889." Northampton. 1890.
- "Annual Report of the Health and Sanitary Condition of the Parish of St. Mary Abbots, Kensington, for 1889." By T. ORME DUDFIELD, M.D. London. 1890.
- "Report of the HOMES FOR INEBRIATES ASSOCIATION and the DALRYMPLE HOME at RICKMANSWORTH, 1889-90." London : H. K. Lewis. 1890.
- "First Report of Dr. J. Francis Churchill's Stœchiological Dispensary for Consumption." London. 1890.
- "Report on the INSTITUTION of DR. BARNARDO'S HOMES for 1889." London : Office of the Homes. 1890.
- "Council's Report of the Public Health Medical Society, ending June 30th, 1890." London. 1890.
- "Calendar of St. Bartholomew's Hospital and College for 1890-91." London. 1890.
- "Howard Association Report, October 1890." London. 1890.
- "Annual Report of the BRISTOL SUNDAY SOCIETY for the Session ending February 16th, 1890."
- "Report on the Works executed by the Honourable Commissioners of Sewers of the City of London, during the year 1889." By WILLIAM HAYWOOD, Engineer to the Commission. March 1890. London. 1890.
- "Annual Report of the St. Marylebone Free Public Library, ending March 31st, 1890." London. 1890.
- "Transactions of the Medical Society of London. Edited by C. A. BALLANEE, M.S., and T. C. FOX, M.B. London : Harrison & Sons. 1890.
- "Monthly Bulletin of the Iowa State Board of Health." Editor, T. F. KENNEDY. Des Moines. 1890.
- "Annual Report of the Lunatic Asylum of County and Town of Nottingham for 1889." Nottingham. 1889.
- "Bericht des Bundesrathes an die Bundesversammlung über die Geschäftsführung und Rechnung der Alkoholverwaltung." 1887-88, und Mai 1890.
- "Annual Report of the Council of the Church of Ireland Temperance Society for 1889."

- "Fourteenth Annual Report of the Australian Health Society, 1888-9." Melbourne: Sands & McDougall. 1889.
- "Report upon the State of Public Health and Sanitary Work performed in Dublin during 1888." By SIR CHARLES A. CAMERON, M.D. Dublin: Falconer. 1889.
- "Annual Report of the London Auxiliary of the U. K. Alliance held in Council Chamber, Exeter Hall." January 30th, 1890. London. 1890.
- "Report of Medical Officers of Health of the Sanitary Condition of Newcastle-upon-Tyne, 1889." Newcastle. 1889.

EXCHANGES.

"The New York Medical Journal," "Journal of Chemical Society," "Journal of Society of Arts," "Chicago Med. Journal and Examiner," "New Orleans Med. and Surg. Journal," "Sacramento Med. Times," "Illustrated Science Monthly," "New England Med. Monthly," "Chemical News," "Clergyman's Magazine," "College and Clinical Record," "Glasgow Med. Journal," "Bristol Med. Chir. Journal," "Australasian Med. Gazette," "Journal of Mental Science," "Quarterly Journal of Inebriety," "Sanitary Journal," "Hygiène Pratique," "Med. Temperance Journal," "The Polyclinic," "Asylum Journal," "Journal of Microscopy," "Longmans' Magazine," "Gentleman's Magazine," "Belgravia," "Chemist and Druggist," "Health," "Sanitary World," "Journal d'Hygiène," "Malthusian," "Animal World," "Scientific American," "Med. Record," "Bulletin de l'Académie de Médecine," "Liverpool Med. Chir. Journal," "Medico-legal Journal," "Hospital Gazette," "Journal Scottish Meteorological Society," "International Journal of the Med. Sciences," "Journal of Heredity," "The Health Journal," "Science Gossip," "The Med. World," "The Photographic News," "Edinburgh Med. Journal," "The Wayfarer," "South African Med. Journal," "Phrenological Magazine," "Med. and Professional Review," "Vegetarian Messenger," "The Analyst," "The China Med. Missionary Journal," Shanghai. "Séances et Travaux de l'Académie des Sciences," No. 5. Par JULES SIMON. Paris: 1887. "The St. Louis Med. and Surg. Journal," "The Journal," "The Canada Health Journal," "The School Board Chronicle," "Our Day," "The Scientific Inquirer," "The Monist," "Public Health," "The Satellite," "The Band of Hope Journal," "University Med. Magazine," "The Journal of Physiology," "New York Abstract," "Murray's Magazine," "Journal of the American Med. Association," "The London Figaro," "Provincial Med. Journal," "Albany Med. Annals," "Accidental Med. Times," "Diet and Hygiene," "The Open Court," "Journal of Physiology," "Journal of Nervous and Mental Disease," "Pictorial World," "Horticultural Times," "Med. Analectic and Epitome," "Journal of Dermatology," "The Med. Record," "Good Health," "Med. News," "Journal of British Dental Association," "American Journal of Med. Sciences," "Drug Topics," "Abkari, Indian Temperance Journal," "Woman," "The Central Review," "Rivista Internazionale d'Igiene," "Australian Med. Gazette," "Boy's Own Paper," "University Med. Magazine," "The Friend of China," "Provincial Med. Times," "The Canine Journal," "The Toccin," "The Hospital," "The Gentlewoman," "The Author."

M.P.
R.

149761

Author Richardson, Benjamin Ward

Title The Asclepiad. Vol. 7

University of Toronto
Library

DO NOT
REMOVE
THE
CARD
FROM
THIS
POCKET

Acme Library Card Pocket

Under Pat. "Ref. Index File"

Made by LIBRARY BUREAU

